Journal of
Experimental Psychology

VOL. 25, NO. 1 JULY, 1939

RETROACTION AND GAINS IN MOTOR LEARNING: I. SIMILARITY OF INTERPOLATED TASK AS A FACTOR IN GAINS[1]

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INTRODUCTION

Scant attention has been directed to the problem of whether or not motor learning[2] shows the same general phenomena of retroaction as do verbal and perceptual learning. It is suggested by Britt (**3**) that retroactive inhibition has been found in both motor and verbal materials, but his conclusion rests mainly on the work of Webb (**25**) and Britt and Bunch (**4**), whose researches were based upon maze performances. An analysis of maze learning shows, however, that it is not so predominantly a motor task as are certain other activities, *e.g.*, pursuit performances. That is, the motor behavior as such which is required for the human being to run a stylus maze, or for the rat to run a floor maze, is quite readily available on the basis of previous learning, and the major problem is for the animal or human being to put

[1] The first of a series of three papers, for which data were collected at the State University of Iowa.

[2] By motor learning is meant skill formation where the cognitive or perceptual element is minimized, but not necessarily eliminated, as, for example, in pursuit performances, or other relatively simple manipulative acts. Serial acts are of interest here; the conditioned response is not considered.

behavior segments together in some sort of pattern which brings him to the experimenter's goal. The human subject, upon *seeing* a stylus maze for the first time, can run it correctly—the motor performance is easy when the maze can be adequately structured perceptually. Husband (**10**) has shown that verbalization, much more often than a motor approach as such, plays a rôle in maze learning for human beings. There seems, furthermore, to be some evidence that the rat, even with his limited perceptual powers in comparison to man, builds up some sort of cognitive structure (Tolman, **21**) or perhaps a general directional orientation (Dashiell, **6**) which as a determining factor in his maze performance may be even more important than the actual acts of running, turning, etc., themselves.

In contrast to these statements about maze behavior, it may be said that a subject, operating a pursuit meter or rotor for the first time, understanding what is expected of him and having acquired many and varied eye-hand coördinations previously, is unable to produce the particular fine motor coördination necessary for good results. Direct cognitive control seems to be less important in this type of performance than in many kinds of behavior, or may prove a handicap, for Renshaw and Postle (**17**) have indicated that subjects given a very complete understanding of the pursuit task and techniques best suited to it do not perform as well as untutored subjects.[3] If a continuum from nearly pure cognitive learning to nearly pure motor learning is conceived of, pursuit performances probably are near the motor end of this continuum, and they should provide a more valid testing ground than maze performance for the question as to the presence of retroaction in motor learning.

Therefore the first of the specific aims of this and a following paper is to determine whether retroactive inhibition, by

[3] Other subjects, actively analyzing their own techniques, were somewhat superior to the untutored group. However, this higher level of motivation or attention produced only moderately better results for the greater output of effort.

A common introspection by those learning pursuit tasks for experimental purposes is that the best technique is to "stand back and let the hand go by itself," a means of minimizing attempts at conscious control of the action.

its usual definition, can be demonstrated in pursuit learning when some other motor task is interpolated between practice sessions on a pursuit rotor. A variety of interpolated tasks must be used in the attempt to answer this question.

The second specific aim, a natural corollary of the first, is to determine the influence of interpolated tasks of varying similarities to the original one upon the retention of the original task. A clear statement as to the usual conception of this problem is given by Britt (**3**):

> At the maximum of dissimilarity (in content, meaning, form, method, operation, environment, etc.) between the two activities, retroactive inhibition may occur. As the degree of similarity of one or all of these factors is relatively increased, the degree of retroactive inhibition also tends to increase. A certain point is eventually reached, however, after which increasing the degree of similarity results in more and more actual identity of the various factors (content, meaning, . . . etc.); and from this point on, *i.e.*, identity of one or more factors, the amount of retroaction tends to decrease, until at the upper limit, actual identity of all the factors, there may be no inhibition at all but simply repetition.

The particular subject-matter to which this statement here is applied is motor learning.

The factor of reminiscence seems not to have been studied sufficiently in relation to amounts of retroactive inhibition in verbal learning. Quite possibly it plays a part in most learning experiments, since Ballard (**1**), G. O. McGeoch (**11**, **12**) McGeoch, Peters and McKinney (**16**), Ward (**24**), and others have found it with varieties of meaningful and relatively meaningless materials, and in both children and adults.[4] Requiring a rather high criterion of perfection on the original task may minimize this problem in the usual retroaction study but in the case of pursuit learning such a procedure is rather difficult if not theoretically undesirable. (There are fundamental reasons why a high criterion of original learning is not demanded in the present experiments. They are presented in a discussion paper, the third in this series.) Since subjects

[4] The only research on reminiscence, by that name at least, in any but verbal or definitely perceptual learning, seems to be G. O. McGeoch's (**13**) study of maze learning by children. She found no reminiscence, but pointed out its significance, in retroaction experiments in general and in establishing the validity of the phenomenon of reminiscence itself, if the gains appeared in motor learning, where rehearsal or practice during a rest period can be prevented.

are interrupted at pursuit learning before the limit of skill possible on the instrument is reached, in most cases, any study of activities interpolated between pursuit practices, and their effect on pursuit learning, is complicated by what seems to be reminiscence, or an analogue of it, in the motor field. The gains in skill (or recovery from fatigue, or growth) demonstrated by Snoddy (**20**), Travis (**23**), Humphreys (**9**), and Doré and Hilgard (**7**) during intervals of no practice clearly show that any learning process interpolated between original pursuit learning and tests for retention of it must occur in relation to the gains, of whatever nature they eventually may turn out to be, which these workers have demonstrated to be present after actual practice has ceased. Reminiscence effects in verbal learning may be relatively fleeting or instable, and perhaps of not such great significance for the problems of retroaction. But in pursuit learning, as will later be shown, gains during periods of no practice are very definite, occur in almost every subject, and very much complicate the study of retroaction.

The third purpose of this series of papers may now be stated as that of determining the effect of interpolated activities upon any facilitation in performance which occurs through growth or other (*e.g.*, recovery) processes during the interpolated period.

In all of the experiments, a fourth purpose will be to compare the sexes. These results will be reported specifically in the second paper of the series. Britt (**3**) has stated that there are no indications that the amount of retroaction is a function of sex differences, although it varies from one individual to another, but not many studies have been directed specifically to this point.

A fifth aim, to be expanded in the third paper of the series, will be to evaluate the results of pursuit learning experiments in the light of certain theories of retroaction, and of gains in motor learning, or, more properly, to evaluate the theories in the light of the present results.

Apparatus and Procedure

The pursuit task chosen as the key task for all the experiments to be reported here was a modification of the Koerth pursuit rotor. It has been described by Beck (2) in complete detail; suffice it to say here that the subject must learn to follow a dime-size target set flush in a phonograph-like fiber disc revolved by an electric phonograph motor. A jointed stylus prevents the subject from applying pressure on the disc or target, so that his main object is to attain skill in the performance of perfect circles in a horizontal plane. The stylus tip must move in a circle about 10 in. in diameter, at a rate of one revolution per sec. A Cenco impulse counter records in 1/120-sec units the length of time per 17-sec trial that the subject succeeds in maintaining contact with the target.

To provide a task which would serve as an interpolated activity *a priori* much like pursuit rotor performance, a simple pursuit meter was designed and constructed by the senior author. It is based on the cam principle which was previously utilized in a complicated sort of pursuit apparatus by Wishart (26). An irregularly shaped large wooden cam, motor driven, activates a lever and spring system which moves a target set on top of the wooden box of the instrument. Movement is in a straight horizontal left-right line, for the subject, and consists of six irregularly spaced reversals of direction within a total excursion of about 6½ in. for each revolution of the cam, which takes about 5 secs. The target of the pursuit meter is like that of the pursuit rotor but slightly less than the size of a dime, and is set in a thin rectangular hard-rubber plate (3 x 9 in.), which is directly connected to the lever system below it, inside the instrument box, and is moved by it. The stylus used with the rotor is plugged into this instrument, and by a simple wiring scheme, the rotor is used for timing and for measuring skill on the linear pattern of the meter in 17-sec periods, just as for the circular pursuit performance.

Spool-packing (as described by Seashore, 19) was chosen as an activity which *a priori* called for some of the same subskills as the pursuit rotor, *e.g.*, finger dexterity, controlled movement of the hand and forearm through space. Superficially the tasks appear quite different but they seemed basically enough alike that spool packing would follow the pursuit meter in the similarity-dissimilarity continuum with respect to the rotor.

What seemed to be a task quite different from pursuit performance was chosen for the third interpolated activity, namely, learning a simple stylus maze by mirror control. The maze pattern as such was apparent to the subject, so that the only item to be learned was reversing directions of apparent movement in the forward-backward dimension, not in the left-right dimension. This task was thought to have less in common with the rotor than did the spool-packing, but later evidence will show this assumption to be at least partially in error.

For the pursuit rotor, pursuit meter, and spool packing tests, standard instructions similar to those used by Seashore (19) and others were used for all subjects. The pursuit performances seem to call mainly for accuracy or coördination, but the spool packing test is a speed test also. The maze instructions were intentionally left rather unspecific; that is, subjects were not told to hurry through the maze, or to attempt to make completely perfect trials, and as a result, from superficial observation by the experimenters, subjects accepted the task as mostly one of avoiding errors, but also one of speed. Therefore, none of the interpolated tasks was a sheer speed task with no demand for accuracy; all called for coördination in varying degrees. For all tasks

a demonstration was given by the experimenter; this has been found the most satisfactory way of introducing the subject to the situation, and it has permitted use of the first trial as a relatively reliable performance. Scores were cited after each trial, and this, plus the challenging character of motor tasks for almost all subjects, insured rather uniformly good motivation.

The statistical reliabilities of pursuit rotor performance and spool packing have been shown in several previous studies to be quite high (*e.g.*, by Buxton and Humphreys, 5) and have not been computed for the present study. The reliability of the present pursuit meter has not been published; for a group of 20 men in this study, the first five-last five coefficient on 15 trials of 17 sec each, uncorrected, was .79; for 20 women, also uncorrected, it was .84. Maze reliabilities have in general been shown to be lower than those of the other tasks here employed; error scores have been given more weight in the minds of the authors than time scores, since errors turned out to have somewhat more correlation with the remaining tests in this experiment.

Subjects

The experimental groups employed are summarized in Table I. Interesting essay material was provided for the reading (control) groups. Conversation during

TABLE I

Experimental Groups Employed and Test Sequences for Each

Female Groups	*N*	12-minute Rotor Ser.	1-minute Rest	12-minute Interp.	1-minute Rest	4-minute Rotor Ser.
Control	30	15 trials, 17 sec each	Conversation with exper.	Reading	Same as first rest period	5 trials, 17 sec each
Meter	20	15 trials, 17 sec each	Conversation with exper.	15 trials, 17 sec each	Same as first rest period	5 trials, 17 sec each
Spool	20	15 trials, 17 sec each	Conversation with exper.	15 trials, 20 sec each	Same as first rest period	5 trials, 17 sec each
Maze	20	15 trials, 17 sec each	Conversation with exper.	about 15 trials; varies somewhat	Same as first rest period	5 trials, 17 sec each

Male groups identical with female, except N = 31 in control group.

the one-minute periods was guided by the experimenter away from the significance of the experiment or inquiries as to adequacy of techniques, but not away from inquiries as to relative status of the performance of a particular subject. The latter information was deemed part of the motivation scheme of the experiment. The experimenter arranged to have the total time spent at spool packing match the 12-minute interpolated period of other groups. Since times varied so widely for maze performance, it was necessary to permit variation in the total number of trials, but the number was usually in the neighborhood of 15, and the last five were arbitrarily selected as indicating the level of skill reached; the total time spent in the activity itself was approximately the same as for the other experimental groups, and the total length of the period was held to 12 minutes also.

In order to match means on the last rotor trials of the first series for the various experimental and control groups (each sex matched separately), it was necessary to compute preliminary means and then arbitrarily place subsequent subjects, on the

basis of the last five of their first 15 rotor trials, in experimental groups whose means on the rotor needed to be lowered or raised to match the control groups. This type of selection does not bias the final results. That such a sampling procedure was not entirely satisfactory, perhaps because of the relatively small number of subjects in each group, will be shown in later discussion.

Of the total of 181 subjects, almost exactly half were college students of the winter terms of 1937–8, most of these being sophomores from elementary psychology classes, and the other half were summer session students, usually of somewhat greater age. These two types of subjects were divided almost exactly evenly within each experimental and control group, so that quite comparable populations were present in each.

All testing took place in a room which was fairly large and adequately illuminated, and the testing was shared by the junior authors of this paper and the next in this series. But one person was in the room with the experimenter at a time, with the exception of part of each control group, when one subject practiced on the rotor while another read in the corner of the same room, facing away from the apparatus. This was done to make it possible to secure a larger number of subjects in the same length of time.

Results

The initial abilities of the male experimental groups, as determined from the average length of contact in each of the last five trials of the first series on the rotor (Table II), were fairly well matched by the control group. Not as satisfactory matchings were obtained for the women's groups (Table II). However, in all cases, the control group was the

TABLE II

Initial Ability on the Pursuit Rotor, Indicated by Mean Secs. Contact on Each of the Last Five Trials in the First Series; Critical Ratios for Differences Between Groups

Men

Variable	Mean	SE_m
Control	4.39	.41
Meter	4.00	.48
Spool	4.32	.31
Maze	4.33	.50

Critical ratio, control vs. meter, .63 (48 chances in 100)
control vs. spool, .14 (12 chances in 100)
control vs. maze, .09 (8 chances in 100)
(All CRs favor superiority of the control group).

Women

Control	2.90	.30
Meter	2.82	.49
Spool	2.38	.33
Maze	2.28	.40

Critical ratio, control vs. meter, .15 (12 chances in 100)
control vs. spool, 1.56 (88 chances in 100)
control vs. maze, 1.24 (68 chances in 100)
(All CRs favor superiority of the control group).

superior on initial ability, and this fact may be utilized in interpreting amounts and directions of change for the various groups.[5]

The question as to whether or not retroactive inhibition exists in motor learning as it usually is found in verbal or more nearly perceptual learning is at least partially answered in the graphs of Fig. 1.[6] It may be seen that in every experimental group, regardless of the type of interpolated activity, the re-test (which usually would be termed a retention test, even after the relatively short period of 14 minutes) indicated a gain over status at the end of the original practice series on the rotor. That the gains in the experimental groups are not merely the result of large positive transfer from the interpolated activities is shown by the fact that the control groups, who spent their interpolated time reading, gained markedly also. If these gains are interpreted as being analogous to reminiscence in verbal learning, it seems to be difficult to explain them fully in the light of Woodworth's (27) conclusion after a review of several studies of reminiscence:

> All the facts of reminiscence, it would seem, can be explained by the reviewing which sometimes takes place, by the varying chances of recall and conditions of recall, and by the condition of readiness into which previously learned material is put by the effort to recall it. The assumption of a long continued process of consolidation seems unnecessary.

Any evidence of retroactive inhibition described here must be stated in terms of differing amounts of gain during the interpolated period, as affected by different types of activity. This may be termed "relative retroaction," as distinguished from the usual implications of the term retroaction (the latter is used quite synonymously with retroactive inhibition here). For this purpose we may compare the final scores of the vari-

[5] The standard deviations for all groups are quite large, which, in view of the small Ns, prevents very significant critical ratios and calls for conclusions in terms of trends only in these studies.

[6] The usual logarithmic curve of pursuit learning in the early stages is not present here. Rather, a straight line comes closer to fitting the data of the first 15 trials for all groups, the curves for women rising somewhat less sharply in each case, from a lower beginning point. It may be that the fairly continuous practice, with brief rest periods, did not permit as great an amount of improvement between trials as is usually found.

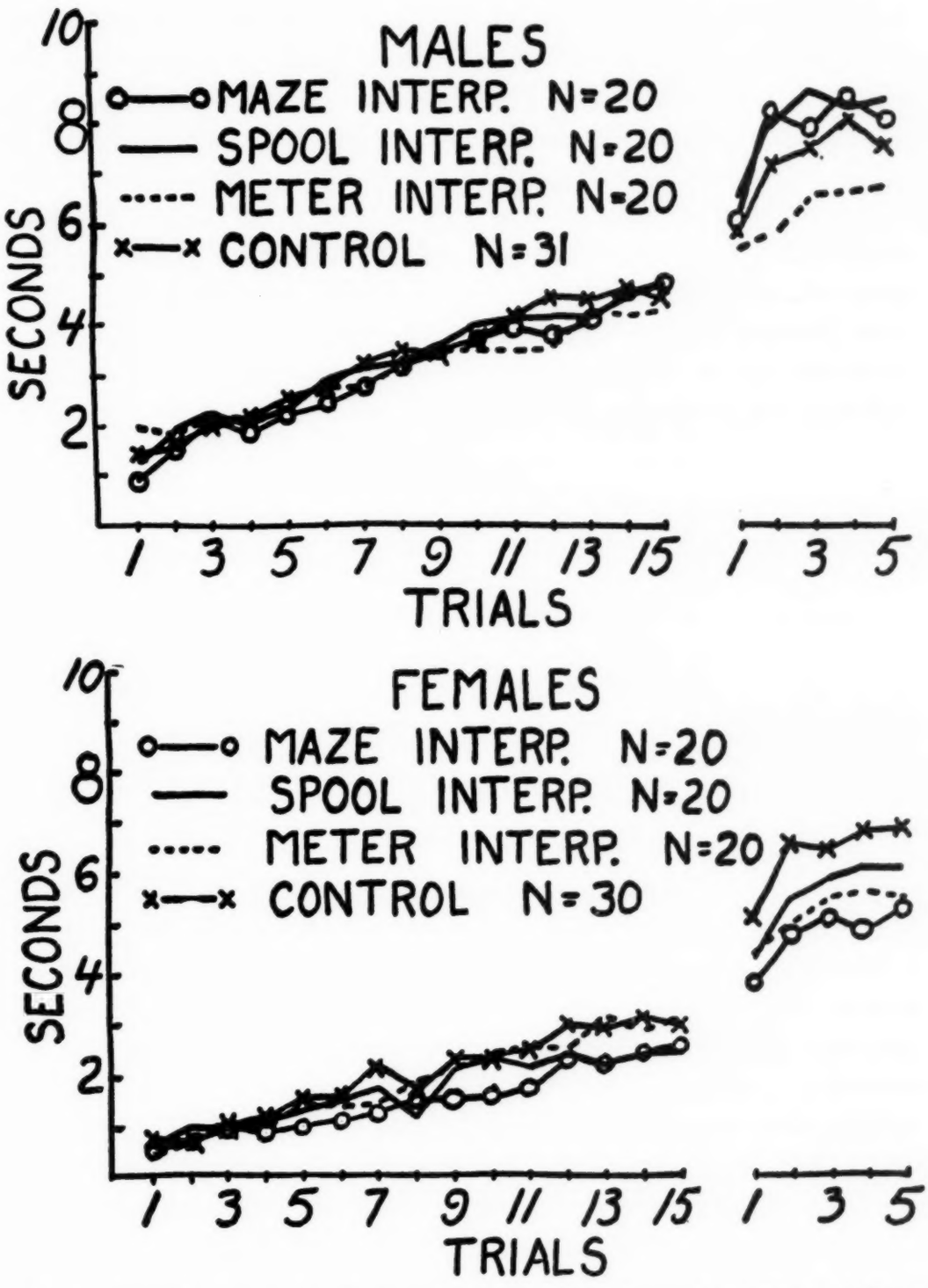

FIG. 1. Learning curves for all experimental groups. Successive ordinates indicate number of trial; abscissæ indicate time in seconds of contact.

ous experimental groups with the control groups and with each other (in the light of their initial matching). The similarity factor thus may be studied as in the conventional retroaction experiment.

It will be seen from Table III that the *a priori* expectations as to amounts of similarity between the different tasks and the rotor did not occur, at least by the correlational criterion, probably in part because of the small numbers of cases within each experimental sub-group. This table shows three somewhat unexpected facts, the high correlation between maze errors and rotor score for women, the absence of correlation between the meter and the rotor for men, and the low correlation between spool packing and rotor for women. (The question as to the suitability of correlation techniques for defining psychological similarity of tasks will not be discussed

TABLE III

CORRELATIONS BETWEEN THE ROTOR AND VARIOUS INTERPOLATED TASKS

Variable	MEN		WOMEN	
	r with last five of first rotor series	*r* with return to rotor	*r* with last five of first rotor series	*r* with return to rotor
Meter	.07	.12	.33	.55
Spool	.51	.45	.23	−.05
Maze time	−.09	−.05	−.20	−.17
Maze errors	−.10	−.23	−.41	−.63

here. Gengerelli (**8**) has utilized the method and given reasons for his adoption of it. In the present study correlation is used as a supplement to the *a priori* analysis of the experimental tasks, which can not so readily be made in terms of stimulus components in pursuit tests as it can in the case of nonsense syllable or number lists.)

Keeping in mind the correlations in Table III, the comparisons of skill on return (or retention) trials on the rotor for different experimental groups may be made, each control group being used as a reference group for that sex. Table IV shows that the maze and spool groups among the men had a higher mean after the interpolated period than did the control group. The meter group, however, did not end as high as the control. For the women we may note that the final status of the control group was higher than all of the experimental groups.

The comparisons in Table IV must, however, be evaluated in terms of the somewhat faulty matching of the groups for initial ability, as shown in Table II. Thus we see that the superiority of the reading group over the maze group among the males, on initial ability (8 chances in 100), is reversed at the final testing, so that the maze group is superior (48 chances in 100). The initial ability of the male controls was greater than that of the spool packing group also (12 chances in 100) and this status is reversed on the final testing (80 chances

TABLE IV

ABILITY ON THE ROTOR AFTER THE INTERPOLATED PERIOD; AV. TIME IN SECS ON EACH OF FIVE RETURN TRIALS; CRITICAL RATIOS FOR THE DIFFERENCES BETWEEN GROUPS

MEN

Variable	Mean	SE_m
Control	7.21	.47
Meter	6.24	.48
Spool	7.91	.30
Maze	7.69	.58

Critical ratio, control vs. meter, 1.44 (86 chances, control)
control vs. spool, 1.26 (80 chances, spool)
control vs. maze, .65 (48 chances, maze)

WOMEN

Control	6.32	.33
Meter	5.15	.59
Spool	5.62	.52
Maze	4.85	.56

Critical ratio, control vs. meter, 1.72 (92 chances, control)
control vs. spool, 1.13 (74 chances, control)
control vs. maze, 2.26 (98 chances, control)

in 100). The initial ability of the control group was greater than that of the male meter group (48 chances in 100), and this relationship was made more definite in the final testing (86 chances in 100 for superiority of the control group). We may summarize the changes in the male groups as follows: the maze and spool packing groups gained more in their interpolated period than did the control (reading) group. Only the meter group gained less than the control.

If reference now is made to Table III, it may be seen that only spool packing shows a correlation with the rotor which

might (on the basis of previous research) indicate similarity enough between rotor performance and interpolated task [7] to lead to any expectation of facilitation, in the interpolated period, greater than that shown by the control group. The maze group, which also improved more than the controls, shows low correlation between maze performance and the rotor, and the meter group, the only one to gain less than the controls, shows approximately the same correlation between interpolated activity and original task as the maze group. Thus far, the correlational check upon similarity of interpolated task and comparative amounts of gain (or relative retroaction) seems to be confusing rather than enlightening.

If the comparisons between control and experimental groups of women, as shown in Table IV, are now analyzed as were the final standings of the men, it is evident that none of the experimental groups had a final status higher than that of the controls However, the control group, initially, superior to the spool packing group (88 chances in 100), at the final testing was relatively less superior (74 chances in 100). The control group was superior to the maze group initially (68 chances in 100) and more so finally (98 chances in 100), and the same state of affairs existed for the control group as compared to the meter group (12 chances in 100 initially, 92 chances in 100 finally). Our interpretation may be, then, that the amount of facilitation showed by the control group during the interpolated reading period exceeded that showed by the meter and maze groups. In other words, meter and maze performance, the first quite definitely and the second less so, have a relative retroactive effect in preventing as full a facilitation as occurred in 12 minutes of reading and two minutes of conversation. Spool packing, however, seems to increase the amount of gain somewhat, as it did for the men.

Referring again to the correlation coefficients in Table III, we see that the meter and maze performances correlated rather definitely with rotor performance for women, and they did not permit full facilitation in comparison with the control

[7] In general, correlation between motor abilities is low, so that in the present papers such terms as 'relatively high' or 'close' correlation may be used where the actual coefficient may not seem high in comparison with coefficients in other fields.

group. The spool packing group, showing rather low correlation with rotor performance, gained more during the interpolated period than did the control group. Thus one might infer tentatively that, for the women, the amount of relative retroaction varies with the similarity of interpolated and original task, as might be expected from the theoretical statement of Robinson (**18**).

The correlational analysis may be summarized as follows: the meter interfered with full facilitation or gain, somewhat for the men and more for the women, and it correlated at a very low level with the rotor for men and relatively closely for women; spool packing facilitated gains more than reading for both sexes, somewhat more for men than for women, and it correlates at a relatively high level with the rotor for men but at a relatively low level for women; the maze showed low correlation with the rotor for men, yet called out greater facilitation than that exhibited by the control group, and for the women, where the correlation between maze and rotor was relatively close, the maze group gained less than the control. It is difficult to make a general formulation of such results.

A different type of analysis, utilizing the correlation technique, is shown in Fig. 2. This approach attempts to combine both the correlational evidence concerning the relation between interpolated task and original task, and an analysis of the influence of the level of skill attained on the interpolated activity.[8] The basis of the analysis which will be made in the next paragraphs is this: the effect of reaching a high level of skill or a low level in the interpolated task, on subjects having a high or low level of skill on the original task, is determined by plotting learning curves for the small subgroups (N = 5 per curve, usually), and is then related to the correlation existing between the two tasks for the group considered as a whole. Thus there are two categories of rotor performance, high and low, and within each a high and low

[8] Apparently only McGeoch (**14, 15**) has paid much direct attention to this problem. In his case frequency of repetition of an interpolated syllable list was assumed to be correlated (closely?) with degree of learning. He found that greater frequency for the interpolated list produced relatively greater retroaction.

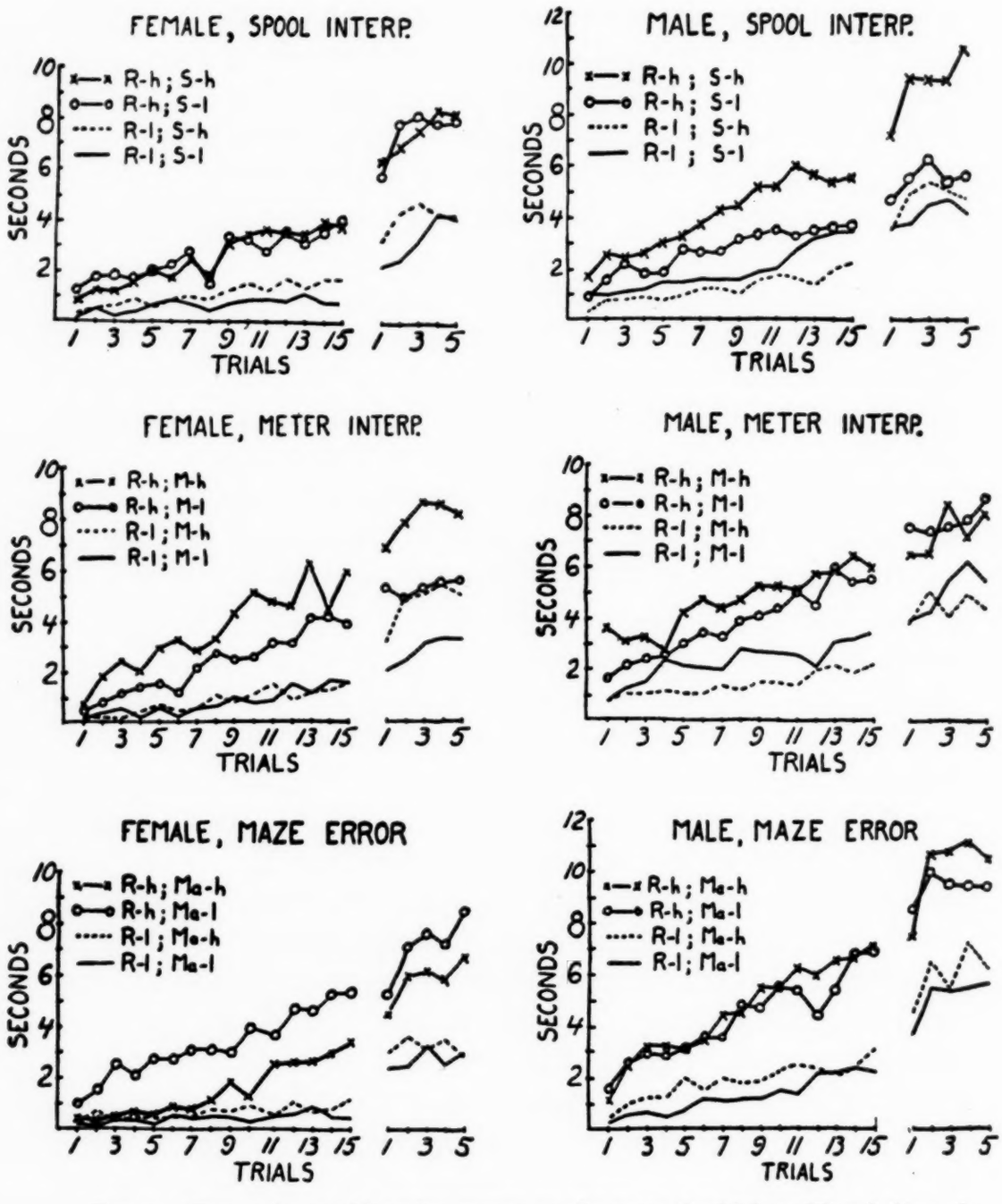

FIG. 2. Comparisons of learning curves on the rotor, for high and low halves of each experimental group, when these halves are split into subjects in high and low halves of the interpolated activity.

group on the interpolated task; this amounts to examining in some detail the variations underlying the gross correlation between tasks.

Spool packing, male: relatively close correlation; rotor-high-spool-high group shows very definite gain, as though spool practice facilitated rotor performance; rotor-high-spool-low group does not show such marked increase. The rotor-low-

spool-high group shows a very definite gain, again as though a large positive transfer were occurring, and the rotor-low-spool-low group does not gain as much. In both extremes of rotor efficiency, then, good spool packing performance definitely seems to aid rotor performance, while poor spool packing does not permit the usual facilitation.

Spool packing, female: relatively low correlation; rotor-high-spool-high group and rotor-high-spool-low group show approximately equal amounts of gain. Of the rotor-low groups, the spool-high shows slightly more gain but the comparative status of the two groups is not nearly as clear as in the case of the males. The general conclusion for this low-correlation group is that there is not much differential effect, upon the original task, of the level of skill attained in the interpolated activity. It was pointed out above that spool packing facilitated more than reading for both men and women, and it now is evident that the effects for men are much more clearly demarcated than for women. This fact presumably is related to the higher level of correlation between spool packing and the rotor for men.

Meter, male: correlation quite low; rotor-high-meter-high group gained less than the rotor-high-meter-low group during the interpolated period, as though reaching a high level of skill on the meter interfered more with the rotor pattern of skill. The rotor-low-meter-high group was initially inferior to the rotor-low-meter-low group, but gained slightly more than the latter during the interpolated period. The meter, it will be remembered, was the only activity which interfered with gains for the males, so that this group gained less during the interpolated period than did the controls.

Meter, female: correlation relatively high; rotor-high-meter-high group gained definitely more than the rotor-high-meter-low group; the rotor-low-meter-high group gained more than the rotor-low-meter-low. Although the total effect for this group, as shown above, is interference with gains during the interpolated period, as compared to the control group, the level of skill in the interpolated activity is shown to be definitely related to the final performance on the rotor.

Maze, male: correlation relatively low; rotor-high-maze-high group shows definitely more gain for the interpolated period than the rotor-high-maze-low group. The rotor-low-maze-high group also shows more facilitation than the rotor-low-maze-low group. The maze group as a whole, as shown above, gained more than the control group in the interpolated period, and the effects of a high order of skill on the maze apparently are facilitative. Yet the correlation between maze and rotor performances is relatively low.

Maze, female: relatively high correlation; rotor-high-maze-high group was by sampling initially lower than rotor-high-maze-low group on rotor performance, but gained relatively more during the interpolated period. The rotor-low-maze-high group showed slightly more facilitation than the rotor-low-maze-low group, so that the general positive transfer from a definitely correlated activity shows up rather consistently in the maze group of females.

These analyses are summarized in section 3 below.

Conclusions

On the basis of the above presentation of results the following conclusions, all of them but tentative, may be formulated:

1. Pursuit learning does not, like verbal learning, show a drop in performance level after interpolated practice on vary-

ing types of motor tasks. Rather, no matter what the interpolated activity, in the present experiment, a definite gain occurs.

2. Women show a relative retroaction, in that certain types of interpolated activity prevent the appearance of as large a gain as that shown by a control group which read during the interpolated period. The pursuit meter had the strongest effect of this type, and the maze next. Spool packing, however, produced a greater gain than that shown by the control group. Only the meter produced relative retroaction for men. The maze and spool packing performances seemed to produce a greater amount of gain for the men than simply reading in the interpolated period.

3. Taken in its gross aspects, the correlation between interpolated activity and original task is in this study no indication of whether the interpolated activity will increase or lessen the amount of gain on the original task. However, when the level of correlation between the two tasks is related to an analysis of groups with regard to level of skill attained in the original task *and* level attained in the interpolated task, there is some indication of the following: (a) where there is definite correlation between the activities for the group as a whole, good performance on the interpolated task improves either good or poor performance on the original task, while poor performance on the interpolated task has little influence on the original task. If anything, poor performance (with its possible effects of inducing emotional disturbances, etc.) may prevent the full amount of gain which might occur. (b) Where there is low correlation, there may be no differential effect of the interpolated activity, or good performance on it may hinder gain on the original task and low performance on it may permit greater gains on the original task to appear. (The male maze group is an exception to these statements.)

Certain qualifications of these statements, and a discussion of their significance, are reserved for a later paper in this series.

(Manuscript received January 10, 1939)

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THE EFFECT OF VOLUNTARY LEG ACTIVITY UPON THE KNEE-JERK FURTHER EXPERIMENT

BY SARAH DUNLAP CANN[1]

Introduction

In a previous study of voluntary response to percussion of the patellar tendon it was found that practice in kicking backward with the percussed leg or forward with the contralateral leg lengthened the knee-jerk time as measured subsequent to the practice (**2**). The present experiment was planned primarily to measure the thickening of the extensor and flexor muscles in the effort to analyze more fully the complex processes involved in the combination of voluntary movements with the patellar reflex. The method of attack was intensive, and hence it was possible to obtain sufficient data from only one experimental subject in the year allotted to the work. This subject was given extensive practice in simultaneously kicking the percussed (right) leg backward and the contralateral (left) leg forward in response to a sound stimulus presented at an interval before the percussion such that the voluntary movements might occur as nearly as possible simultaneously with the knee-jerk. Records of the knee-jerks alone from both right and left legs were made preceding the practice and at suitable periods during the course of the practice. Further, for purposes of control, the subject was given lengthy series of percussions eliciting the knee-jerk from the right leg without voluntary response, and a second, control, subject was similarly stimulated in series of percussions without voluntary response. These control series bore upon the secondary purpose of the experiment, namely, to investigate the effect of long series of elicitations of the knee-jerk.

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The antecedents of the problem with pertinent investigations by others are presented and discussed in the above-mentioned report.

Apparatus.—The central piece of apparatus was a pendulum photochronograph by which to record simultaneously, through optical and mechanical levers, contraction thickening of the extensor and flexor muscles of both legs, and also the moment of percussion of the tendon.

The subject sat in a special chair, nine inches from front of seat to back. The light-tight booth in which the photochronograph operated was behind the subject, and in front of her was a percussion pendulum for tendon stimulation, and a book rest, with a screen between the book rest and the pendulum, precluding vision of the latter by the subject.

The subject's feet, without shoes, rested firmly on a horizontal platform attached to the chair, at such height as to make the knee angle slightly less than 90 degrees. This adjustment was constant throughout the experiment. The feet were fastened to the platform by soft straps, rendering them immovable, with the heels approximately 11.5 inches apart. This support and fixation of the feet avoided the numbing which occurs when the leg is supported under the knee, with the foot free. Rods attached to the chair kept the legs in proper position without discomfort.

The optical lever system was provided by four small concave mirrors attached to axles from each of which projected a lever of balsa wood. Two of these levers pressed down, one on the extensor muscle of each leg; and the other pair pressed upwards on the flexor muscles. The extensor levers pressed on the position of maximal bulging of these muscles, determined by palpation when the muscles contracted, and the flexor levers were directly under them. Each lever was pressed into the muscle by a rubber band passed over it and attached to a fixed support. Greater pressure was necessary on the flexor levers, because of the thickness of tissues over the muscle. The light sources were two straight filament lamps, one for each leg, the images of the filaments reflected by the mirrors being focused across a vertical slit mounted in the front of the camera booth, the adjustment being such that the images assumed the positions (lowest) right extensor; left extensor; right flexor; (highest) left flexor. The pendulum bob, carrying the sensitized paper, moved close behind the slit, so that the light beams traced lines as the pendulum swung.

Across the slit was a grid of fine copper wires 1 mm apart, the shadows of which, cast by a clear lamp with a horizontal filament, served as ordinate units for the measurement of deviations of the record lines. For time measurements two mercury tubes, flashing on opposite phases of the 60-cycle current, and so illuminating the slit 120 times per second, made dark lines across the photographic film (8⅓ *sigma* intervals). These lines are light in the figures reproduced herewith, since the figures are negatives. The tubes were vertical and hence cast no shadows of the grid. The pendulum was held at one end of its swing by an electromagnet, the breaking of the current through which released the pendulum for an exposure.

In its swing the chronograph pendulum actuated a switch which closed a circuit through a buzzer, serving as an auditory stimulus. This switch was adjusted in position so that the stimulus was given at the proper length of time before the percussion to the tendon.

The percussion pendulum was 30 cm long, with a bob weighing 119.5 gm, cylindrical in form with a flat front surface 1.5 inches in diameter which struck a wooden

bar ⅜ inch wide taped horizontally across the patellar tendon. A brass plate on the outer surface of this bar made electrical contact with the pendulum bob, establishing current through a magnetic marker. This marker, fastened to the front wall of the chronograph booth, with its motile needle extending across the slit, recorded, by its shadow on the photographic film, the moment of the percussion on the tendon. The latent period of the magnetic marker was constant, and therefore negligible.

The percussion pendulum was held at the top of its swing by an electromagnet which could be adjusted to any angle along a protractor scale. A single switch broke the circuit through the magnet holding the chronograph and through a relay controlling the magnet of the percussion pendulum, the delay of the relay being adjusted so that the percussion stroke was delivered at the beginning of the photographic record.

The right and left pairs of recording beams entered the slit at slightly different angles, and did not strike the sensitive paper on the same vertical line, being on either side of the shadow of the marker needle. The corrections for these angles are given in the section on *the Curves*.

In measuring the records, the reader considered the knee-jerk to begin at the point where the curve first rose perceptibly, ignoring certain small rises in the nature of a recovery from the jar due to the blow, which did not continue into a definite contraction. The height was measured from the resting level before the blow to the first leveling of the contraction curve, to the nearest ½ mm. The time could be measured to the nearest ¼ of a time unit, or to 1/480 second, and was transformed into the nearest sigma value.

Experimentation and Statistical Data

Subject A (the experimental subject) came for sittings of an hour each, usually numbering six a week, every week from April 15 until June 14, and Subject C (the control subject) at about the same frequency from May 8 until June 14, as indicated in Tables I, II and III. The final days were employed in testing the apparatus under different conditions to insure that it introduced no errors. Another subject, B, came for one hour on five successive days but became ill and was replaced by C. Records were taken on her of both knee-jerks and conformed with those obtained on Subjects A and C. Subject A was a young woman graduate student in psychology, 5 feet 2 inches tall and weighing about 135 pounds. Subject C was a woman college graduate, not acquainted with Subject A. She was 5 feet 5¼ inches tall and weighed 117 pounds. Neither subject had previously been used in a similar experiment.

There were four types of series in the experiment: series of records of the right and the left knee-jerks for comparison,

TABLE I

Subject A

Sitting	KJ Records Before Practice Series				Voluntary Kicks							KJ Records After Practice Series				
					With KJ Blow											
					Recorded											
	R	L	R	L	Total N	% with KJ inh.	% with KJ betw.	% with KJ uninh.	N Not Recorded	Cumulative N	Without Blow	R	L	R	L	R
Apr.																
15 A.M.		10	21	12												
P.M.	11	22	11													
17 A.M.	12	22	11													
P.M.		11	23	11												
24 A.M.		10	10		23	6	0	94	0	23	0					
P.M.		10	10		4	0	25	75	0	27	59					
26 A.M.	10	10			25	15	38	47	3	55	0					
P.M.	10	10			10	10	0	90	0	65	37					
27 A.M.		10	10		47	21	32	47	28	140	0					
P.M.		10	10		48	27	23	50	50	238	1					
May																
1 A.M.					39	49	6	43	40	317	1	10	10			
P.M.					38	37	8	55	40	395	2	10	10			
3 earlier A.M.					40	73	0	27	40	475			10	10		
A.M.					60	23	15	62	62	597						
6 A.M.					65	29	12	59	65	727						
P.M.					50	30	14	56	52	829		10	10			
8 A.M.					60	50	18	32	60	949						
P.M.					60	57	10	33	60	1069						
10 A.M.					25	28	16	56	75	1169	2	10	10			
P.M.					30	13	30	57	90	1289	2					
13 A.M.					33	33	21	46	97	1419	2					
P.M.					25	8	28	64	75	1519	2		10	10		
15 A.M.					33	6	24	70	98	1650						
P.M.					33	3	12	85	97	1780						
17 A.M.					25	24	28	48	75	1880			10	10		
P.M.					18	11	22	67	36	1934		12	24	24	12	
18 P.M.					31	42	39	19	99	2064						
20 P.M.					25	44	40	16	75	2164	1	10	10			
21 P.M.					33	30	49	21	94	2291						
later P.M.					33	15	67	18	100	2424						
22 A.M.					18	50	22	28	36	2478		12	24	24	12	
24 P.M.					33	58	33	9	97	2608						
27 P.M.					33	64	36	0	97	2738						
28 A.M.					25	64	36	0	75	2838		10	10			
P.M.					33	52	45	3	97	2968						
29 A.M.					25	71	26	3	105	3108						
P.M.					10	80	20	0	0	3118		20	40	40	20	
31 A.M.					34	65	12	23	105	3257						
P.M.					10	60	20	20	0	3267			20	40	40	20
June																
3 P.M.					35	83	14	3	105	3407						
later P.M.	20	40	41	19												

series of voluntary kicks, and series of repeated elicitation of the right knee-jerk.

Tables I to III indicate the order in which various types of series were made at each sitting. The procedure during all series was consistent. While knee-jerk records were being taken, the subject read a book. In the series of voluntary kicks there was always a spoken 'ready' signal. Otherwise, the series of knee-jerks and voluntary kicks were comparable. When reading, the subject was instructed to keep her attention on the book. The instructions for the voluntary kicks were to kick the right leg back and the left leg forward in

TABLE II

SUBJECT A

Sitting	Repeated Elicitation of RKJ			KJ Records after Practice Series					
	N Recorded	N Not Recorded	Cumulative N	R	L	R	L	R	L
June									
4 P.M.					11	22	22	22	11
6 P.M.				11	22	22	22	21	10
7 A.M.	33	97	130						
P.M.	25	75	230		10	10			
10 P.M.	35	105	370						
later P.M.	35	105	510						
11 P.M.	25	75	610			10	10		
later P.M.	35	105	750						
12 P.M.	35	105	890						
later P.M.	25	75	990		10	9			
13 P.M.	35	105	1130						
later P.M.				20	40	40	20		
14 P.M.					10	20	20	10	

response to the buzzer, and the subject was informed that the object of the kick was to inhibit the knee-jerk. The subject sat perfectly still. She informed the experimenter if anything seemed unusual, that the experimenter could not know about. There was little to report, and there was little conversation. The experimenter came out of the booth one or more times during every long series to make sure that everything was in adjustment.

A large electric fan was kept going during all the sittings, to mask any possible sound cues. The subject knew the

purpose of the fan. On June 14 both subjects reported that they had received no cues, and this was confirmed by their inability, on specific test, to recognize any noises made by the apparatus.

TABLE III
SUBJECT C

Sitting	Repeated Elicitation of RKJ			KJ Records after Practice Series				
	N Recorded	N Not Recorded	Cumulative N	R	L	R	L	R
May								
8 P.M.				15	30	15		
9 P.M.					12	24	23	12
10 P.M.				14	29	28	14	
13 P.M.					11	22	11*	
14 A.M.	74		74		10**	10**		
15 A.M.	51	50	175		10**	10**		
16 P.M.	40	40	255	10	10			
17 P.M.	40	40	335	10	10			
18 A.M.	40	40	415		10	10		
20 A.M.	60	60	535					
21 A.M.	65	65	665					
22 A.M.	51	51	767	10	10			
P.M.	60	60	887					
23 A.M.	60	60	1007					
24 A.M.	25	75	1107	10	10			
28 A.M.	30	90	1227					
29 A.M.	33	97	1357					
P.M.	26	76	1459		9	11		
30 A.M.	33	94	1586					
31 P.M.	33	94	1713					
June								
1 A.M.	25	75	1812		10	10		
3 A.M.	33	97	1942					
P.M.	18	36	1996	12	24	24	12	
4 A.M.	35	105	2137					
5 P.M.	35	105	2277					
6 A.M.	35	105	2417					
7 A.M.	35	105	2557					
P.M.				20	40	39	19	
8 A.M.					20	40	40	20
10 P.M.				20	40	40	20	

* A series of voluntary kicks followed.

** These records preceded the practice series.

Up to June 14 neither subject had guessed the purpose of the experiment. This was partly because Subject A had been told that it was not an attempt at conditioning, and Subject C that the effect of fatigue was not being sought. These statements were, of course, true in the narrowest sense of the terms. Subject A suspected that a comparison was being

made between distraction from and attention to the reflex, the former being a natural and the latter an unnatural situation. She also wondered if reflexes increase with use. She had an idea that her right knee-jerk was always stronger than her left, which is borne out by the records. Her right tendon was observed by the experimenter to be harder than her left, a fact which she said might be due to her always starting to walk with the right leg. She wondered if a more exciting section of the novel she was reading would cause a change in the knee-jerk. This factor was not controlled. Subject C was less interested in the experiment. She noticed that, in the course of the sittings, she became less sensitive to a knee-jerk stimulus, but could not suggest a reason.

It was not considered necessary to record every kick in practice series or in the corresponding control series. The kicks recorded in such cases were usually alternates or every third or fourth kick, that is, evenly distributed through the series. The number actually recorded is indicated in Tables I to III. Several extra blows were always given at the beginning of any right or left group, and sometimes during a series.

A few records were discarded because, due to some accident, they could not be read. These accidents were: too early fall of the blow, or the recording lines becoming superposed, or exposure of the record before developing. In certain records height or latency alone was legible.

It will be seen from Table I that Subject A's knee-jerk was tested before, during and after practice of the right back kick with the left forward kick in the attempt to inhibit the right knee-jerk. The tests consisted of comparisons of the right with the left knee-jerk, and were always arranged to eliminate the effects of the temporal order of right and left blows. Table I indicates the order of procedure, and the effect of the voluntary kicks on the knee-jerks in practice. In Table IV the results are tabulated. With the height (H) and the latency (L) are given the right-left difference and its reliability wherever it might be of interest. The mean variation is increased by averaging records from different days.

TABLE IV

SUBJECT A

Sitting		Height in mm.				Latency in sigma			
		RKJ	LKJ	Diff. R − L	D/σD	RKJ	LKJ	Diff. L − R	D/σD
April 15 A.M., P.M.	Range	1 — 8	0 — 7			37.5-48	46 — 71		
	Med.	3	3	0		44	52	8	
	Mean	4.0	3.3	.7		43.2	51.9	8.7	
	M.v.	1.6	1.6			1.2	2.8		
	N	*43*	*43*			*35*	*36*		
17 A.M., P.M.	Range	1 — 10	1 — 11			33 — 50	42 — 52		
	Med.	6.5	6	.5		40	46	6	
	Mean	6.5	6.0	.5	1.2	40.4	45.5	5.1	8.5
	M.v.	1.5	1.8			1.7	2.0		
	N	*46*	*44*			*45*	*44*		
24, 26, 27, A.M. and P.M.	Range	1.5-14.5	0.— 13.4			31 — 50	35 — 58		
	Med.	8	5.75	2.25		40	44	4	
	Mean	8.4	6.0	2.4		39.0	44.0	5.0	
	M.v.	3.4	2.3			4.0	3.6		
	N	*60*	*60*			*59*	*57*		
May 1 A.M. and P.M., 3 earlier A.M.	Range	2 — 9	1 — 13			35 — 52	33 — 56		
	Med.	6.25	7.5	−1.25		40	40	0	
	Mean	5.9	7.6	−1.7	2.1	41.6	43.1	1.5	1.8
	M.v.	1.8	3.2			4.3	6.3		
	N	*30*	*30*			*29*	*24*		
6 P.M., 10 A.M., 13 P.M., 17 A.M.	Range	8 — 15.5	7.5-15			31 — 37.5	31 — 37.5		
	Med.	12.5	14.75	−2.25		35	33	−2	
	Mean	12.3	14.1	−1.8	3.6	34.9	33.6	−1.3	3.2
	M.v.	1.5	2.3			1.5	1.6		
	N	*40*	*40*			*40*	*40*		
17 P.M.	Range	14 — 21	9 — 14.5			25 — 37.5	21 — 40		
	Med.	17	12.75	4.25		33	35	2	
	Mean	17.1	12.5	4.6	11.5	32.9	34.8	1.9	3.2
	M.v.	1.4	1.2			1.8	1.7		
	N	*36*	*36*			*36*	*36*		
20 P.M.	Range	13 — 18.5	12.5-16			31 — 37.5	29 — 33		
	Med.	16	15	1		33	33	0	
	Mean	15.7	14.7	1.0	2.0	33.5	32.0	−1.5	1.9
	M.v.	1.2	.8			1.5	1.2		
	N	*10*	*10*			*10*	*10*		
22 A.M.	Range	7 — 18.5	3 — 15			31 — 40	33 — 44		
	Med.	14	11.5	2.5		34	37.5	3.5	
	Mean	13.8	10.6	3.2	5.3	34.2	37.9	3.7	5.3
	M.v.	2.0	2.1			1.5	2.6		
	N	*36*	*36*			*36*	*31*		

TABLE IV (*Continued*)

Sitting		Height in mm.				Latency in sigma			
		RKJ	LKJ	Diff. R − L	D/σD	RKJ	LKJ	Diff. L − R	D/σD
May 28 A.M.	Range	5 — 8	8 — 13.5			37.5–46	31 — 42		
	Med.	6	12	−6		42	37.5	−4.5	
	Mean	6.2	11.3	−5.1	7.3	42.1	36.6	−5.5	4.3
	M.v.	.9	1.6			1.9	2.5		
	N	*10*	*10*			*10*	*10*		
29 P.M.	Range	5 — 15	4 — 12			33 — 44	35 — 52		
	Med.	10.5	7.5	3		37.5	44	6.5	
	Mean	10.1	7.3	2.8	9.3	38.3	42.6	4.3	7.2
	M.v.	1.8	1.2			2.3	2.9		
	N	*60*	*60*			*60*	*60*		
31 P.M.	Range	5.5–17.5	.5–14			31 — 40	29 — 58		
	Med.	12	5	7		35	42	7	
	Mean	11.6	5.7	5.9		35.8	41.9	6.1	
	M.v.	2.0	2.9			1.9	5.4		
	N	*60*	*60*			*60*	*60*		
June 3 later P.M.	Range	3 — 15	.5–15.5			29 — 42	31 — 54		
	Med.	10	6.5	3.5		35	40	5	
	Mean	9.6	7.0	2.6		35.1	40.7	5.6	
	M.v.	1.5	3.0			2.2	3.4		
	N	*59*	*61*			*56*	*59*		
4 P.M.	Range	5 — 15.5	2 — 12.5			31 — 44	37.5–54		
	Med.	11.5	6	5.5		35	46	11	
	Mean	11.8	6.3	5.5		35.8	45.7	9.9	
	M.v.	1.6	2.0			2.7	3.8		
	N	*44*	*44*			*42*	*44*		
6 P.M.	Range	5.5–17	1.5–13			31 — 44	35 – 62.5		
	Med.	11	6.5	4.5		35	42	7	
	Mean	10.9	6.6	4.3		36.2	42.9	6.7	
	M.v.	2.2	2.1			2.4	3.1		
	N	*52*	*54*			*50*	*51*		
7 A.M., P.M.	Range	8.5–21				31 — 40			
	Med.	14.25				35			
	Mean	14.5				35.3			
	M.v.	2.0				1.5			
	N	*58*				*58*			
10 P.M., later P.M.	Range	8.5–19				29 — 44			
	Med.	15				35			
	Mean	15.0				35.3			
	M.v.	1.4				1.9			
	N	*70*				*70*			

TABLE IV (*Continued*)

Sitting		Height in mm.				Latency in sigma			
		RKJ	LKJ	Diff. R − L	D/σD	RKJ	LKJ	Diff. L − R	D/σD
June	Range	6 -- 20				33 -- 44			
11 P.M., later	Med.	15				37.5			
P.M.	Mean	14.5				36.6			
	M.v.	2.6				2.2			
	N	*59*				*58*			
12 P.M., later	Range	6 -- 19				29 -- 40			
P.M.	Med.	12.5				35			
	Mean	12.5				34.9			
	M.v.	2.0				1.7			
	N	*60*				*60*			
7 P.M.	Range	9.5- 18	2 -- 11.5			35 -- 40	33 -- 60		
11 P.M.									
12 later	Med.	14	7	7		37.5	40	2.5	
P.M.	Mean	13.5	7.1	6.4	9.1	36.7	40.7	4.0	3.3
	M.v.	1.8	2.0			1.5	3.6		
	N	*29*	*30*			*29*	*30*		
13 P.M.	Range	5 -- 16				33 -- 44			
	Med.	11				37.5			
	Mean	10.9				36.9			
	M.v.	2.7				2.9			
	N	*35*				*35*			
13 later	Range	2.5- 21	.5- 14			33 -- 54	33 -- 50		
P.M.	Med.	11	8	3		37.5	37.5	0	
	Mean	11.4	7.5	3.9		39.7	38.4	−1.3	
	M.v.	3.5	2.8			3.7	4.1		
	N	*59*	*56*			*59*	*55*		
14 P.M.	Range	5 -- 16	3 -- 16			29 -- 37.5	29 -- 42		
	Med.	10.75	11.75	−1		33	33	0	
	Mean	10.8	11.6	−.8		33.2	33.9	.7	
	M.v.	1.4	2.3			1.7	2.4		
	N	*30*	*30*			*30*	*30*		

There is a temporary lengthening of the right knee-jerk latency and decrease in the height of contraction shown in the averages of those comparison series which were taken immediately after a practice series and which were very short. Those of more than 22 records: May 17, P.M., do not show this change. This difference may be seen from the averages to be independent of the fact that records which are taken during

the first few minutes are always on the average greater in height and less in latency than those taken later.

The percentage of records in which the knee-jerk appears definitely inhibited by the voluntary kicks is given in Table I. Among those considered as doubtful, and those where the knee-jerk is present, are undoubtedly some in which there was undetected inhibition. The criteria are discussed below in the section on *the Curves*. The effect of practice as shown in the difference between the right and left knee-jerks is greatest on May 28, just preceding which there had been a large percentage of knee-jerks inhibited by the voluntary kicks. The difference is not due to the fact that the right knee-jerk series was taken before the left, for that would produce an opposite difference. This effect seems to be but transitory and does not show on May 29, presumably because the series on that day was long. In every case when a long series of comparison records was taken, the effect was cancelled; but it seems to appear as soon as even slight inhibition is present. Between May 10 and 17 difficulties in the apparatus caused most of the time intervals to be such that there was but little inhibition, though some effect of the practice remains. This raises the question whether actual inhibition of the knee-jerk in practice as seen in the curves is necessary to produce the effects found.

Judging from the averages of April 17 compared with those of May 29 and later, it might be said that adaptation to the experimental situation with repeated elicitation of both knee-jerks tends to make the right knee-jerk greater in height and less in latency as compared to the left knee-jerk. The records of April 15 are not included in this consideration because they were inaccurate. This strengthening of the right knee-jerk is visible in the longer series and is an effect opposed to the transitory inhibition of the right knee-jerk. It may or may not be due solely to the repeated percussion of the right knee without stimulation of the left, since it does not disappear in some series on days when equal numbers of right and left knee-jerks were elicited (June 4 and 6) although it becomes less in some such series (June 3, later P.M. and June

13, later P.M.). These are the only long comparison series that were taken at a late hour, in the same afternoon as other series, and should not be directly compared to the others. Whether this relative increase of the right knee-jerk is due to the voluntary practice cannot be determined, since the increase was in evidence after the voluntary practice was discontinued. Since repeated elicitation of the right knee-jerk caused no such consistent change in Subject C, this change is probably due to the voluntary kick situation, but could easily be fortuitous.

As a control, right knee-jerks were elicited from Subject A with the same procedure as was used when she was making voluntary kicks. No consistent effect can be found, as can be seen from Tables II and IV. The cause of the simultaneous changes in both right and left knee-jerks, which seem to be gradual, is not known. The room temperature did not vary much, although the weather changed progressively; and there were no relevant changes in the subject's mode of life. In Tables IV and V the comparison series, which are averaged separate from the repeated elicitation series, may be recognized by the inclusion of both right and left knee-jerk data.

The data from Subject C are in Table V. No consistent effects of repeated elicitation of the right knee-jerk are visible.

The correlation between the height and the latency of the knee-jerk was calculated from all the knee-jerk records. For Subject A, right leg, 971 records, it was − .62; left leg, 672 records, − .80; Subject C, right leg, 1462 records, − .70; left leg, 466 records, − .64; all records, totaling 3571, − .75.

The Curves

Further information may be obtained from a study of the types of curves obtained. The contraction of a flexor sends its curve downwards, and the contraction of an extensor sends its curve upwards. Motion of a leg up or down sends both its curves correspondingly up or down. The motion of the flexors is magnified less than that of the extensors because the levers on the flexors were longer. The necessary corrections for points on the curves, to render them commensurable with

TABLE V

SUBJECT C

Sitting		Height in mm.				Latency in sigma			
		RKJ	LKJ	Diff. L − R	D/σD	RKJ	LKJ	Diff. R − L	D/σD
May 8, 9	Range	.5–18	6 – 16.5			37.5–69	35 – 54		
	Med.	6	12	6		50	42	8	
	Mean	6.2	11.8	5.6	9.3	50.3	42.7	7.6	8.4
	M.v.	3.3	2.2			4.8	3.2		
	N	*65*	*65*			*65*	*65*		
10, 13	Range	1.5–22.5	2.5–20			37.5–58	35 – 48		
	Med.	12	12	0		46	44	2	
	Mean	12.1	12.0	−.1		45.6	43.3	2.3	
	M.v.	3.8	2.7			3.9	3.2		
	N	*63*	*65*			*63*	*65*		
14, 15	Range	3 – 11	7 – 20			40 – 56	37.5–48		
	Med.	8.75	11.25	2.5		49	44	5	
	Mean	8.2	11.5	3.3		49.9	44.1	5.8	
	M.v.	2.0	2.3			3.7	2.1		
	N	*20*	*20*			*20*	*20*		
14	Range	3.5–18				37.5–58			
	Med.	11				46			
	Mean	10.7				46.3			
	M.v.	3.0				3.7			
	N	*74*				*74*			
15	Range	.5–12				42 – 65			
	Med.	6.5				50			
	Mean	6.0				50.9			
	M.v.	2.4				3.8			
	N	*51*				*51*			
16, 17, 18	Range	0 – 21				33 – 60			
	Med.	8.5				46			
	Mean	8.8				45.3			
	M.v.	3.2				5.0			
	N	*120*				*120*			
16, 17 18	Range	.5–14.5	1 – 21			40 – 73	35 – 56		
	Med.	7	7	0		48	49	−1	
	Mean	7.1	8.2	1.1		48.7	47.8	.9	
	M.v.	3.9	3.7			4.6	3.9		
	N	*30*	*30*			*30*	*30*		
20, 21	Range	.5–19				33 – 60			
	Med.	7.5				48			
	Mean	8.0				47.7			
	M.v.	3.1				3.3			
	N	*125*				*125*			

TABLE V (*Continued*)

Sitting		Height in mm.				Latency in sigma			
		RKJ	LKJ	Diff. L − R	D/σD	RKJ	LKJ	Diff. R − L	D/σD
May 22 A.M., P.M., 23	Range	0 – 23.5				35 – 85			
	Med.	12				45			
	Mean	11.9				44.7			
	M.v.	3.3				4.1			
	N	*171*				*171*			
24, 28, 29 A.M., P.M.	Range	1 – 20				35 – 58			
	Med.	7.25				48			
	Mean	8.0				48.0			
	M.v.	3.3				4.2			
	N	*114*				*114*			
30, 31	Range	.5–20				37.5–67			
	Med.	6				52			
	Mean	7.3				51.4			
	M.v.	4.2				5.6			
	N	*66*				*65*			
June 1, 3 A.M., P.M.	Range	0 – 18				35 – 85			
	Med.	5.5				52			
	Mean	6.7				51.2			
	M.v.	3.6				5.0			
	N	*75*				*75*			
May 22 A.M., 24, 29 P.M., June 1	Range	0 – 16	2.5–21			37.5–67	33 – 54		
	Med.	6	8	2		51	46	5	
	Mean	6.0	9.8	3.8	3.5	51.8	45.3	6.5	4.3
	M.v.	3.6	4.5			6.0	4.9		
	N	*40*	*39*			*40*	*39*		
June 3 P.M.	Range	1 – 18	3 – 21			33 – 54	35 – 60		
	Med.	5.5	9	3.5		50	46	4	
	Mean	6.2	9.3	3.1	3.9	47.7	46.7	1.0	.8
	M.v.	2.3	3.3			4.0	3.7		
	N	*36*	*35*			*36*	*34*		
4, 5	Range	0 – 15				35 – 60			
	Med.	3.75				52			
	Mean	4.6				51.6			
	M.v.	2.7				3.7			
	N	*70*				*68*			
6, 7 A.M.	Range	.5–14.5				35 – 62.5			
	Med.	7				50			
	Mean	8.1				48.9			
	M.v.	4.2				5.4			
	N	*70*				*68*			

TABLE V (*Continued*)

Sitting		Height in mm.				Latency in sigma			
		RKJ	LKJ	Diff. L − R	D/σD	RKJ	LKJ	Diff. R − L	D/σD
June 7 P.M.	Range	.5–20	.5–14			37.5–67	44 –65		
	Med.	6	5	−1		52	54	−2	
	Mean	7.0	5.7	−1.6		51.8	54.2	−2.4	
	M.v.	3.7	2.8			4.7	3.8		
	N	*59*	*59*			*59*	*59*		
8, 10	Range	0 –19.5	.5–22			35 –94	35 –58		
	Med.	4	9	5		54	42	11	
	Mean	5.0	9.2	4.2		56.0	42.6	13.4	
	M.v.	2.0	3.5			5.4	3.8		
	N	*120*	*119*			*119*	*119*		

the blow-signal, is ½ of a time unit toward the right for the left leg curves, and ¼ of a unit toward the left for the right leg curves.

Figures 1 and 2 are typical wide knee-jerk records, Fig. 1 a left knee-jerk of Subject B, and 2 a right from C, May 10. In all records, the dark line is the signal for the blow, and starting at the bottom the light lines are consecutively the right extensor, left extensor, right flexor, and left flexor at the beginning of the record. The time of the blow is assumed to be the moment at which the marker begins to move. This assumption was verified by applying the blow directly to one of the axles of the recording system. The blow also shows in the extensor record and, in many records, the flexor, of the leg percussed.

The flexor curve of the leg percussed rises possibly because the knee-jerk moves the whole leg up. The motion appears to be partly a jarring of the part of the leg against which the flexor lever was pressed. It cannot be demonstrated from the records obtained, whether this rise is partly due to relaxation of the flexor. There is also a rise in the flexor curve of the opposite leg, which appears to be due to motion of the leg. These rises are generally greater for wider knee-jerks, and for Subject A alone, greater with the left than the right knee-jerk. The contralateral extensor curve should also show this rise,

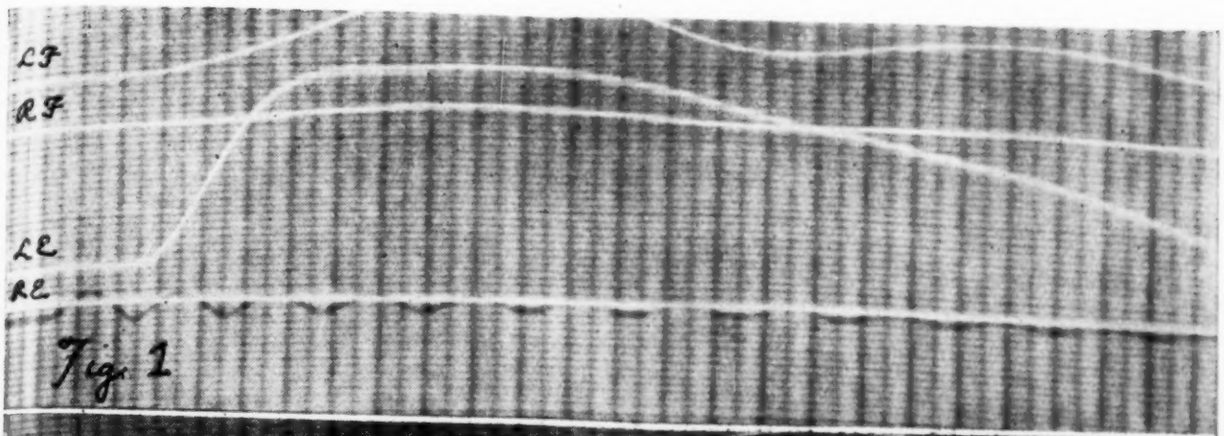

Fig. 1

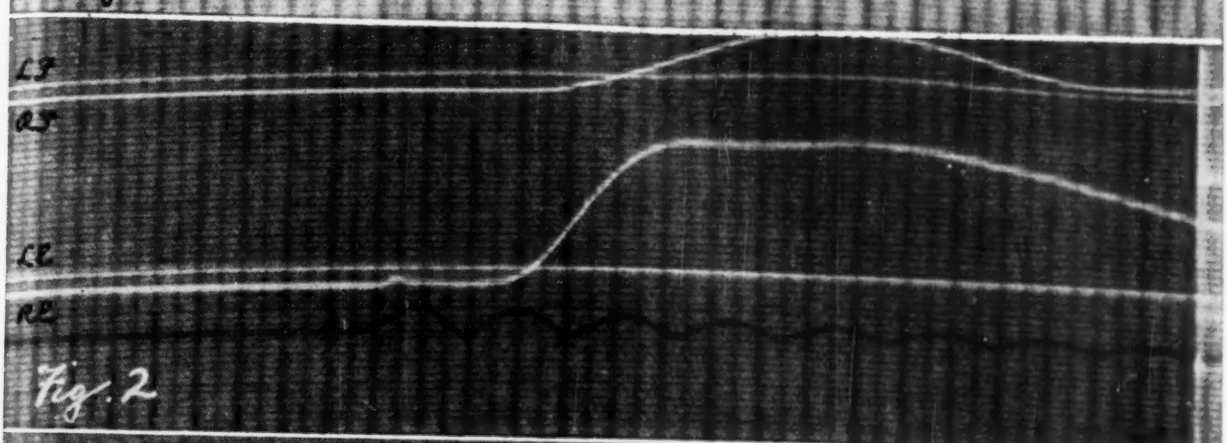

Fig. 2

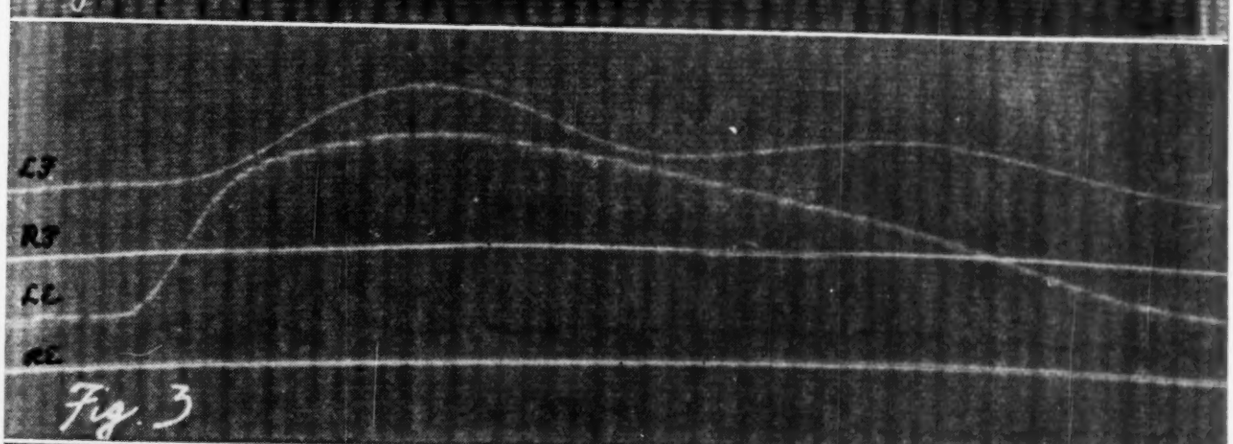

Fig. 3

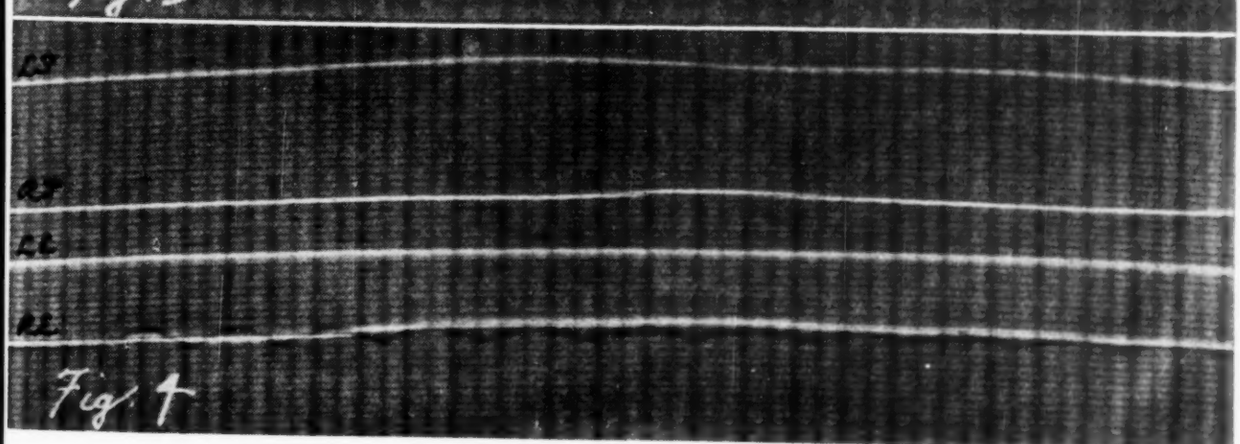

Fig. 4

but shows instead a small downward motion, which is assumed to be due to relaxation and to overcome any upward leg motion. Figure 3, of C, June 10, shows the opposite flexor moving down before it moves up, or presumably contracting. This is typical of many records of both legs, particularly of Subject C, for whom the upward motion of the flexor of the opposite leg is lacking in most cases. When it occurs it is usually with a wide knee-jerk. Figure 4, a right knee-jerk, and 5, a left knee-jerk, from B, April 30, show what is interpreted as a contraction of the flexor of the leg percussed, superposed on the leg-rise; a curve similar to the flexor curves from the leg opposite to the one percussed, exemplified in Fig. 3. Such records appear also in Subject A, in instances to be noted.

The accentuation of leg motion in the flexor record and the secondary rise and fall seen in the latter part of all records appear in the nature of a damped vibration superposed on an upward motion. They are not vibration due to the axles; for the free periods of the axles were between 92 and 95 sigma, and were apparently not affected by the attachment of the levers with their rubber bands. Figure 6 from C shows the course of the vibrations, the blow having been given before the beginning of the record. These vibrations appear to be constant in period for each leg of each subject, and the period is considerably over 100 sigma. Since the flexor levers were pressed upward by rubber bands, no doubt the cause of these vibrations is a bouncing of the lever and band system against the leg.

It may be seen in records of voluntary back kicks with forward kicks of the opposite leg that the flexor curve for the back kick does not descend very far. This is because with the downward contraction of the muscle, the whole leg rises and moves backward. For the extensor lever, while the upward motion is not wide, the backward motion also raises the lever because of the rearward slope of the thigh in the position assumed by the subject. As a result, the extensor curve rises much higher than it does in a voluntary forward kick. (These effects, which are due to the foot being immovable, are much less than those occurring when attempts were made to obtain

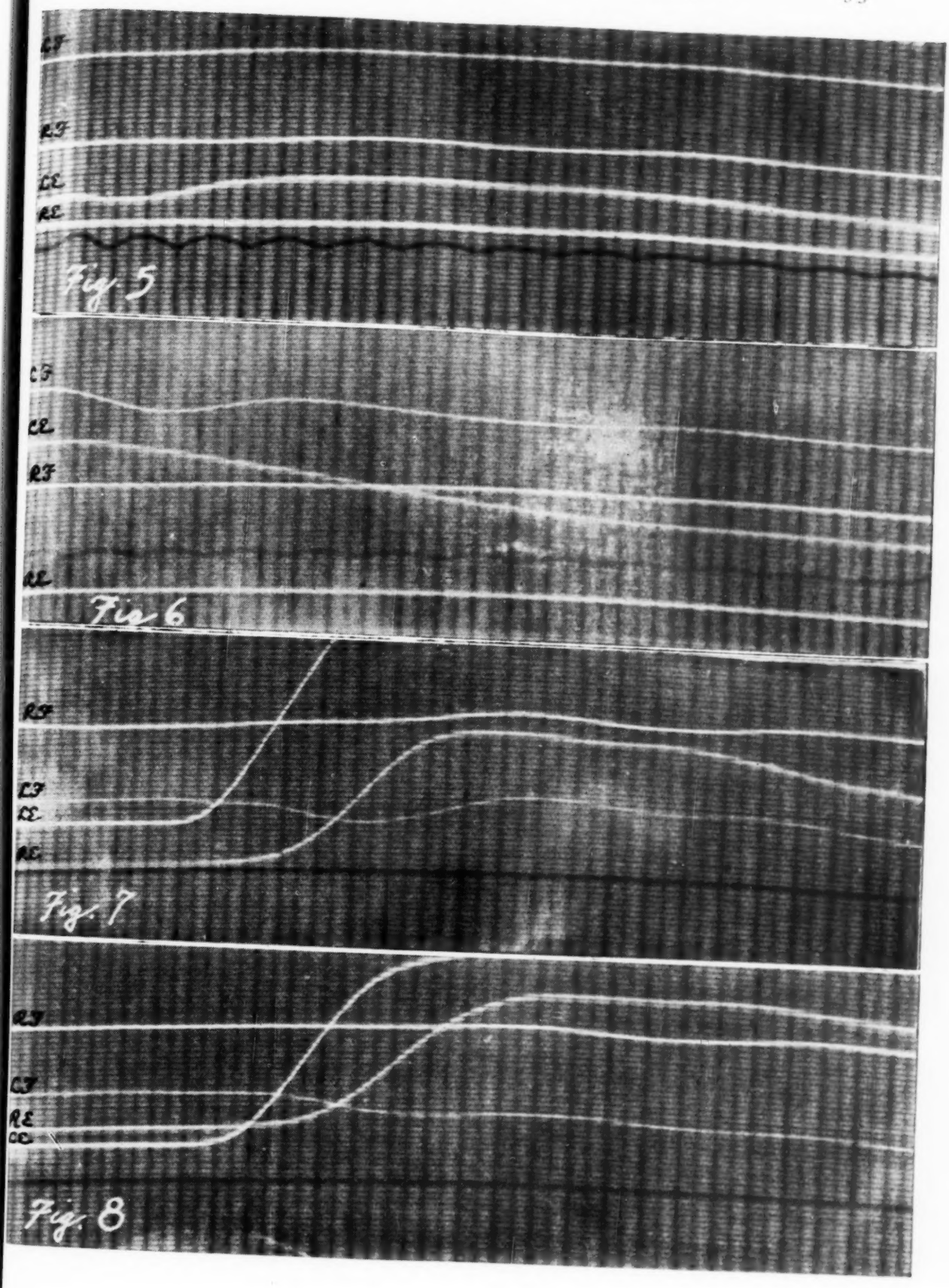
LF
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Fig. 5
LF
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Fig. 6
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Fig. 7
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Fig. 8

records with the foot swinging free, because in the latter case the tendon of the flexor had to rest on the knee-support.) Figures 7 and 8 show the rise of the leg. They are left backward, with right forward, kicks from Subject B. The lines are out of order, being, in Fig. 7, from bottom to top, right and left extensors, left and right flexors; and in Fig. 8, left and right extensors, left and right flexors. Figure 9 is a left backward and right forward kick from C, May 13, and 10 is a right backward and left forward kick from A, April 26. The flexor curves are typical but there was considerable variation among them. Subject A never made very violent left forward kicks, and often the flexor of the left leg contracted. This contraction is discussed below.

Back kicks made by Subject A became gradually more violent, that is, fast and wide, in the series up to April 27. That this was due to the presence of the knee-jerk blow is evidenced by a record on May 31 when the blow was omitted. The contraction and rise of the leg was much less than any other on that day. Figure 11 is a record from May 1 when there was no blow because the percussion pendulum had inadvertently not been raised. Here the voluntary kicks came much later than usual as compared to Fig. 12, the preceding record, in which the back kick is as late as in any record of that sitting. (The blow-signal in Fig. 11 indicates contact at the knee followed by a break due to the jar of the kick.) The inference from these facts is that the knee-jerk blow was an essential part of the stimulus pattern in practice, and conditioning of the back kick to it may be expected. In this connection, compare the right flexor in Fig. 13 with the left flexor in Fig. 14, which are respectively a right and a left knee-jerk from April 27, A.M. There is a right flexor contraction with the right knee-jerk somewhat like that in Figs. 4 and 5, which was seen in all records but two of this sitting, while Fig. 14 is similar to all the left records. The strict criterion assumed for the presence of the flexor contraction with the knee-jerk was that the first rise of the flexor curve be less than the second rise. This contraction appears again in 2 records on May 3, earlier A.M., 2 on May 20, P.M., 5 on

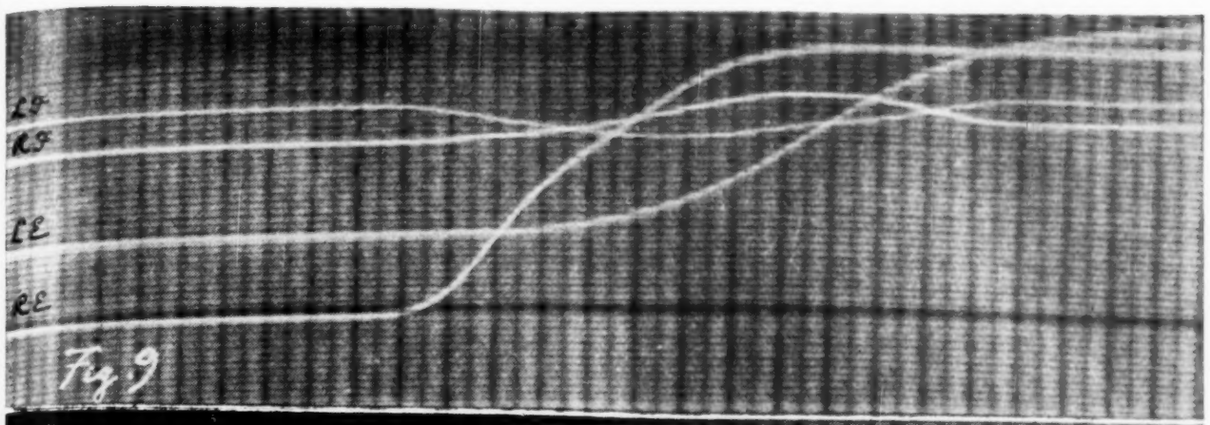

Fig. 9

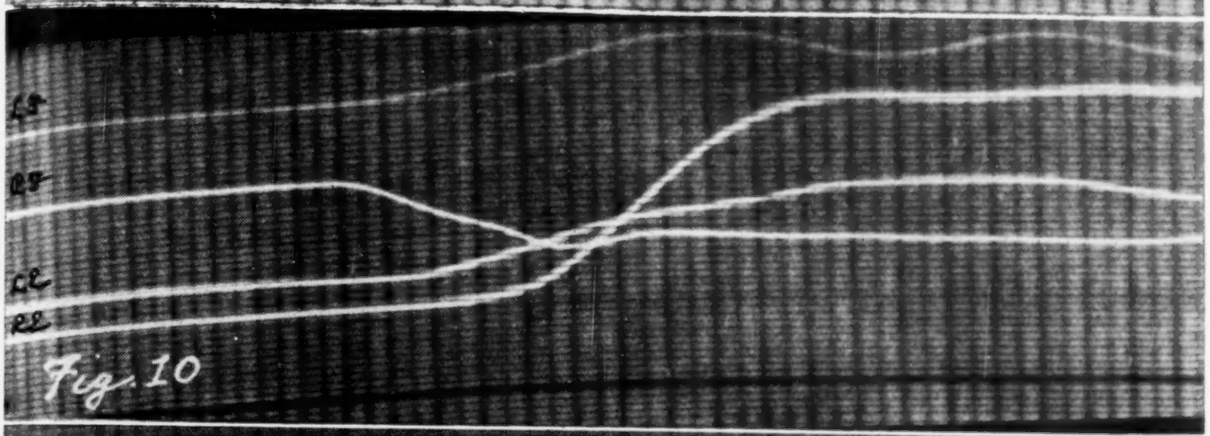

Fig. 10

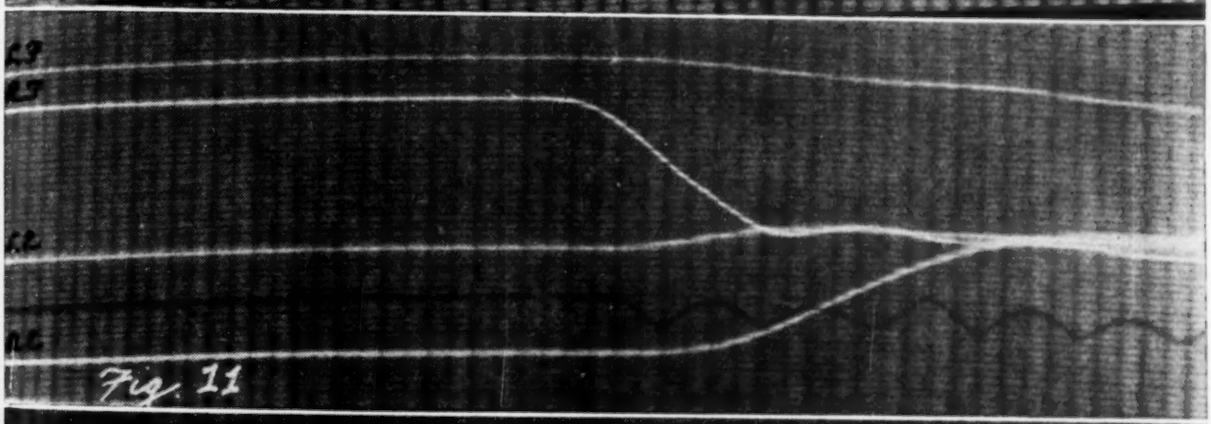

Fig. 11

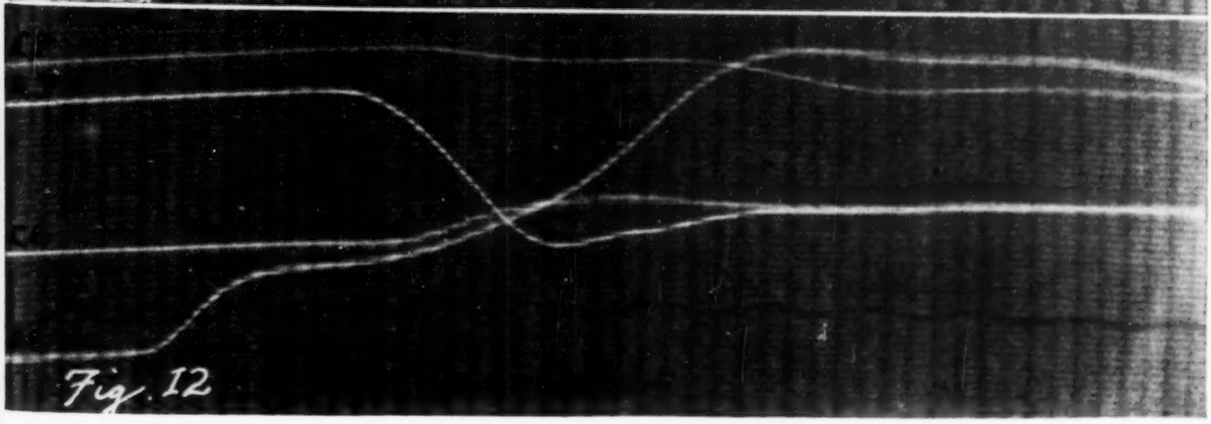

Fig. 12

June 7, P.M., 33 on June 10, P.M., 35 in the June 10, later P.M. series, 34 on June 11, P.M., 9 on June 12, P.M., and 28 in the later P.M. series, 26 on June 13, P.M., with one record not readable, and 24 in the later P.M. series. On June 14 they appeared, although with very small motion, in all the right and 8 of the left records, while the left flexor showed no such peculiarity. This form of right flexor curve is not due to any peculiar action of a lever or axle, for in this sitting the right and left levers were exchanged, and the right extensor with the left flexor axle, and the left extensor with the right flexor axle.

This form of flexor contraction curve was most marked in the early records of a series, when the knee-jerks also averaged the widest, as in Fig. 15. Figure 17, from June 12, P.M., shows a peculiarity that occurred but was not definitely listed as a flexor contraction.

The flexor contraction curve, as described above, seems to appear following elicitation of the right knee-jerk without much elicitation of the left, until on the last day it is well established. Since it appears also in Subject B's first records, it is not necessarily due to the previous back kick practice, although it does not appear in records from Subject C. It may be considered to be a true contraction, since it appears in only one leg, but without action current records its latency cannot be measured, and its cause cannot be definitely stated.

The varying picture presented by the left flexor in all the records from Subject A of voluntary kicks with knee-jerks supplies the best evidence for the interpretation that has been given of the meaning of the flexor curves. In any given curve the rise of the leg cannot be analyzed distinct from contractions or relaxations; but considering all records together it appears that the varying motions of the flexor are superposed on a vibration beginning with a rise, exactly like the curve of the left flexor with the right knee-jerk. This curve is so consistent that it must be of mechanical origin, since the legs are jarred by any kick. Sometimes the left flexor also is seen to contract in these records. Its contraction is superposed on the other motion, but is seen not to be a

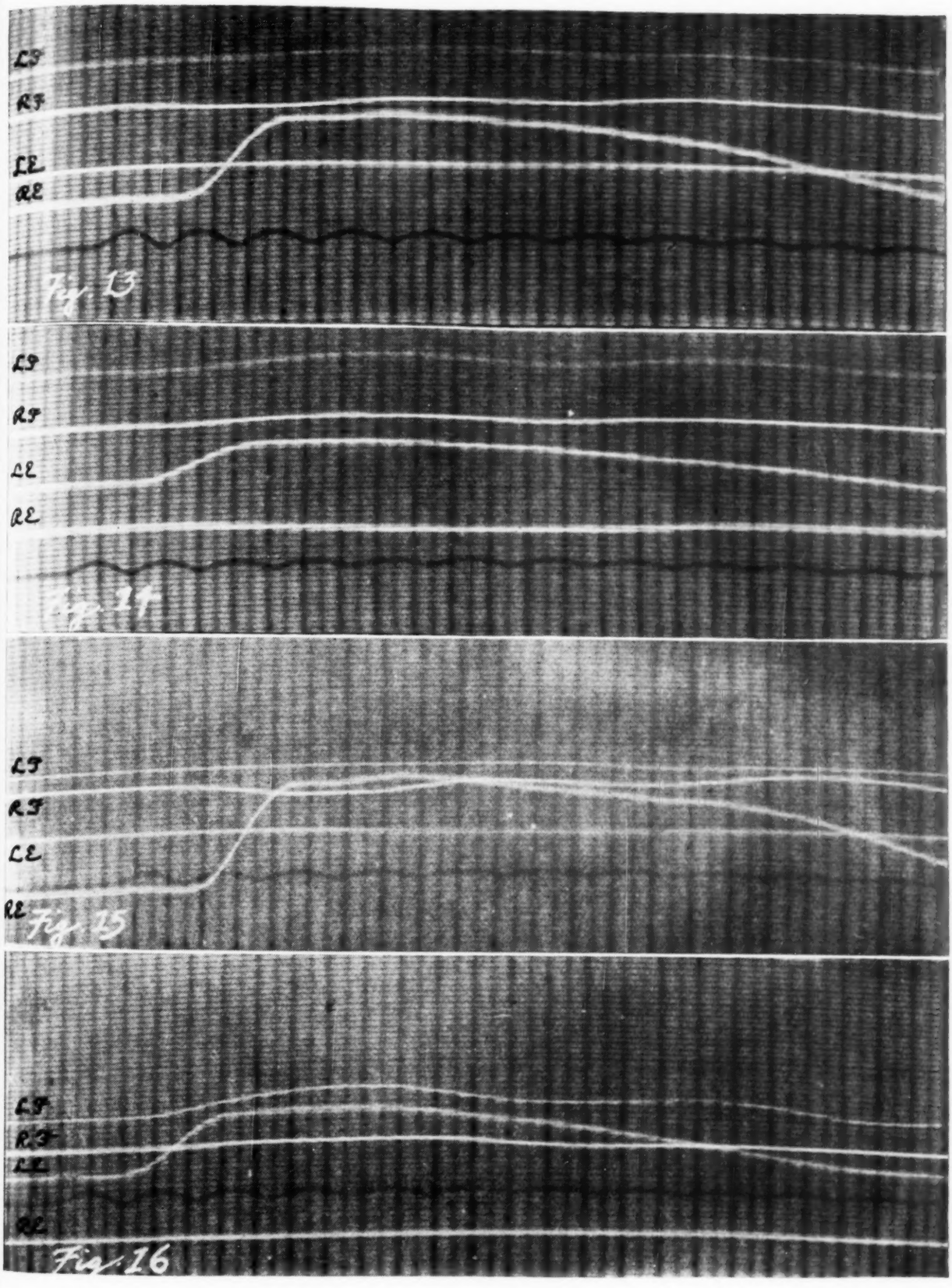
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Fig. 13
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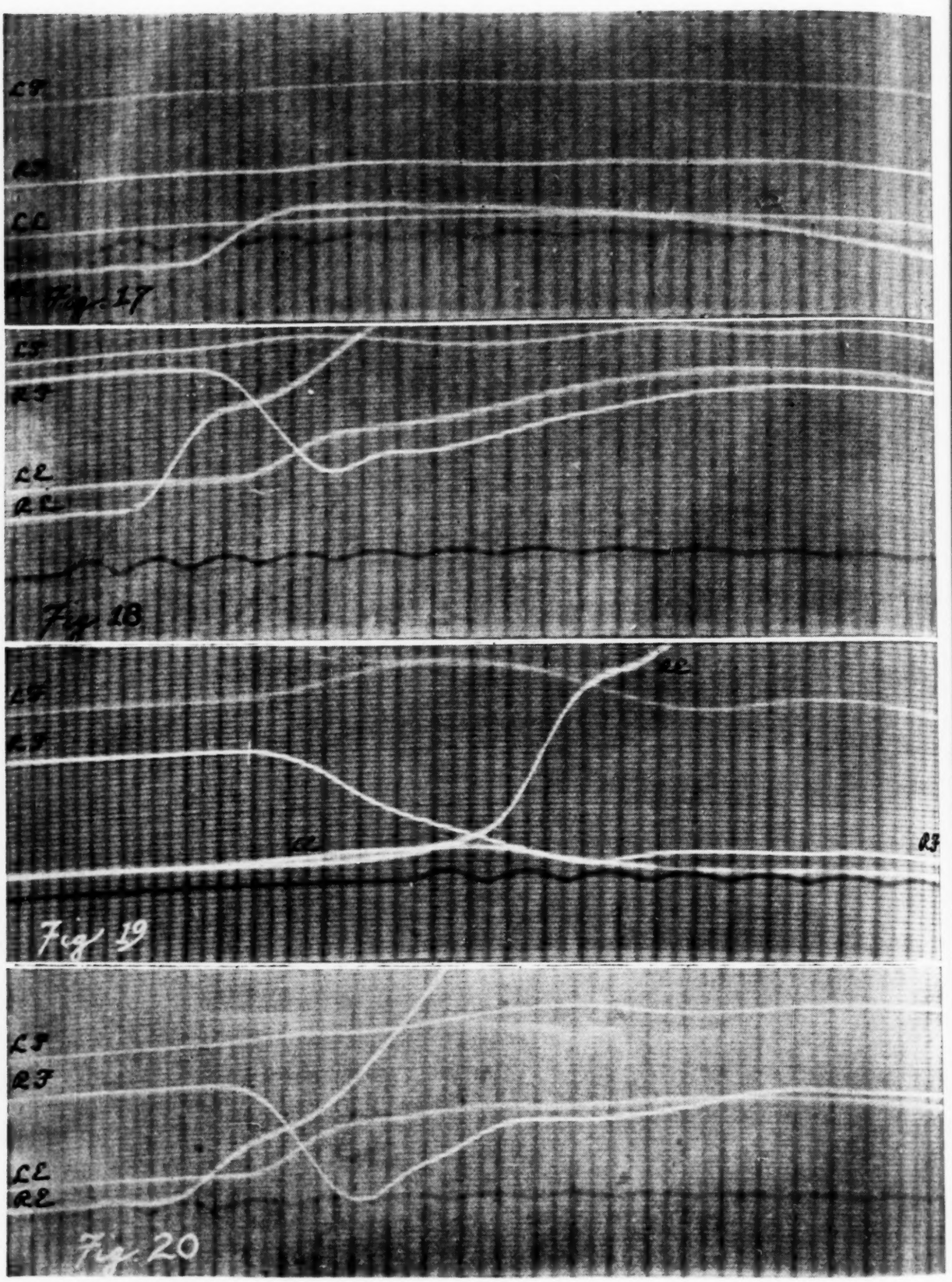
Fig. 16


L3
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LL
Fig. 17
L3
R3
LL
RL
Fig. 18
L3
R3
RL
R3
Fig. 19
L3
R3
LL
RL
Fig. 20

part of it because the two do not always occur in the same temporal relation. Figures 7 and 8, from Subject B, show some contraction of the right flexor (top line) when she was kicking that leg forward and the other backward without a knee-jerk blow; Fig. 9, from C, shows only leg motion in the right flexor curve with the same kick.

Figure 18, which with the remainder of the figures is from Subject A, shows when compared to 19 and 30 a contraction of the left flexor damping the first rise. Figure 20 indicates that the left leg moved down as a whole, since both left curves descend. Whether the left flexor contracted is doubtful in Figs. 21, 22, and 23. Further downward motion of the left flexor in the records of Subject A seems to coincide with several factors. Sometimes it occurs with continuance of the right flexion, sometimes with descent of the left extensor. Sometimes the same curve is visible in the left flexor while the extensor rises. Figures 12, 18, 24, 25, 26, and 27 show varying degrees to which the flexor contraction overcomes the rises due to the voluntary kicks and knee-jerk. Some motion of the left leg may also be present.

This flexor contraction was present from May 1 onward. Two records appear to lack it on each of the days May 6, 8, 11, and 28; 4 to 10 appear to lack it on each of the days May 10, 21, 24, and 29. On May 31 it was present in only 3 records, and on June 3 in none.

There is no reason to expect the left flexor to relax when the left extensor contracts voluntarily; rather the presence of flexor contraction is to be expected. The absence of relaxation of an antagonist is visible in many records when the right flexor contracts less violently, thus not immediately raising the leg. It may be argued that there is simultaneous rise and relaxation, but this would not make so smooth a curve. Figures 19 and 25 illustrate this point. In 19 the two extensor curves are superposed, but it is the left one which rises first.

The percentage of cases in which there was definitely some inhibition of the knee-jerk was obtained without reference to the other curves on the records, except to the back kick curve to determine when to expect the rise of the leg. Sample

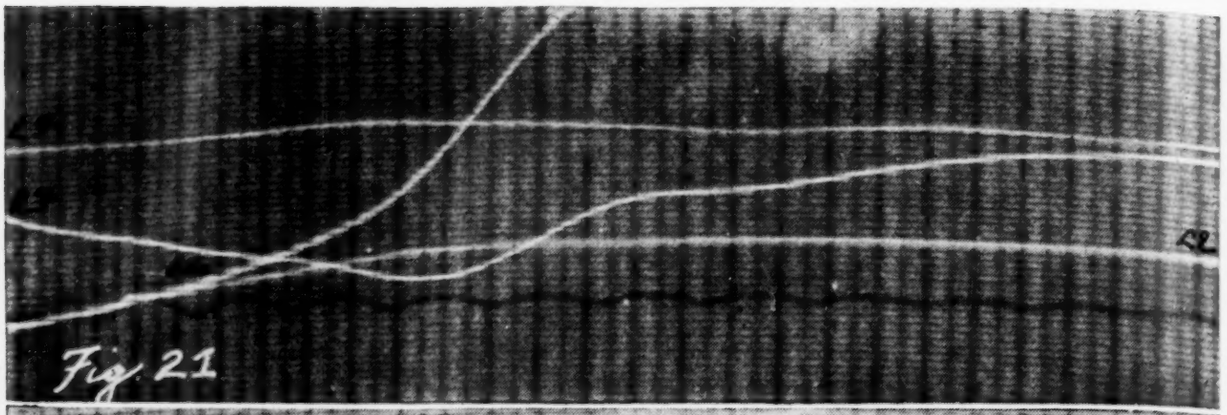


Fig. 21

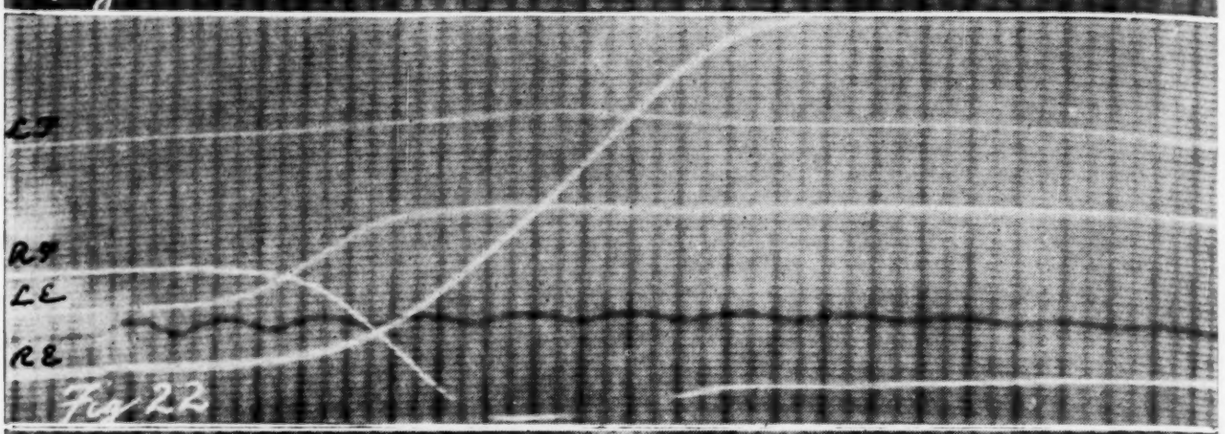


Fig. 22

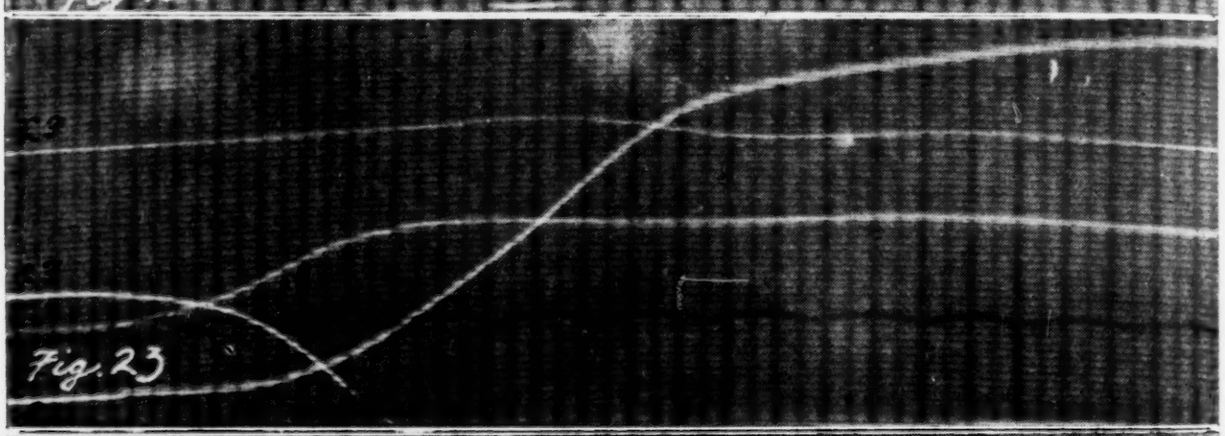


Fig. 23

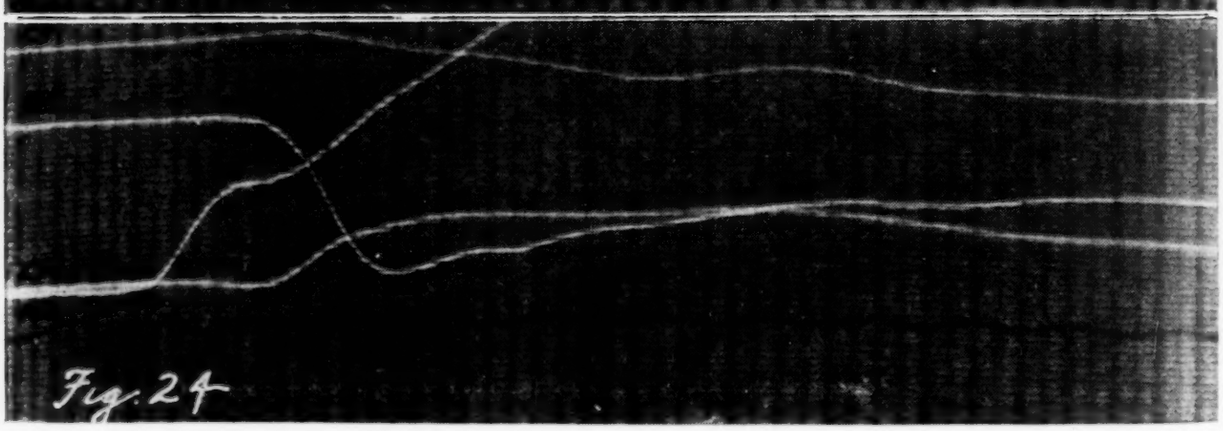


Fig. 24

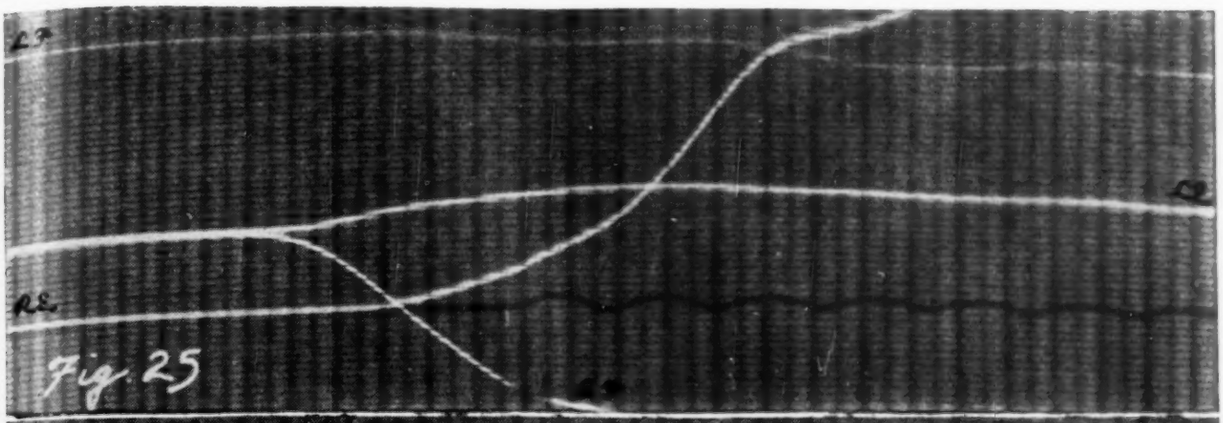


Fig. 25

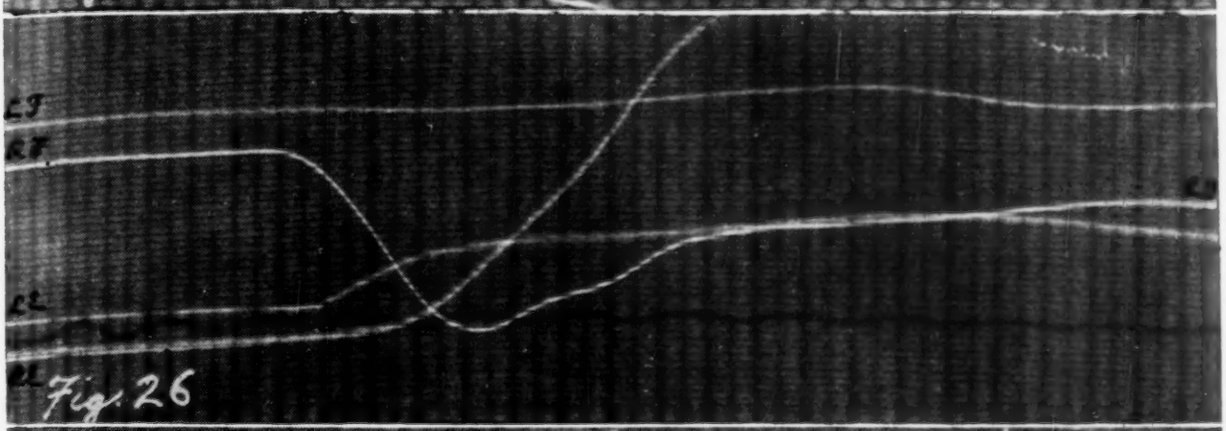


Fig. 26

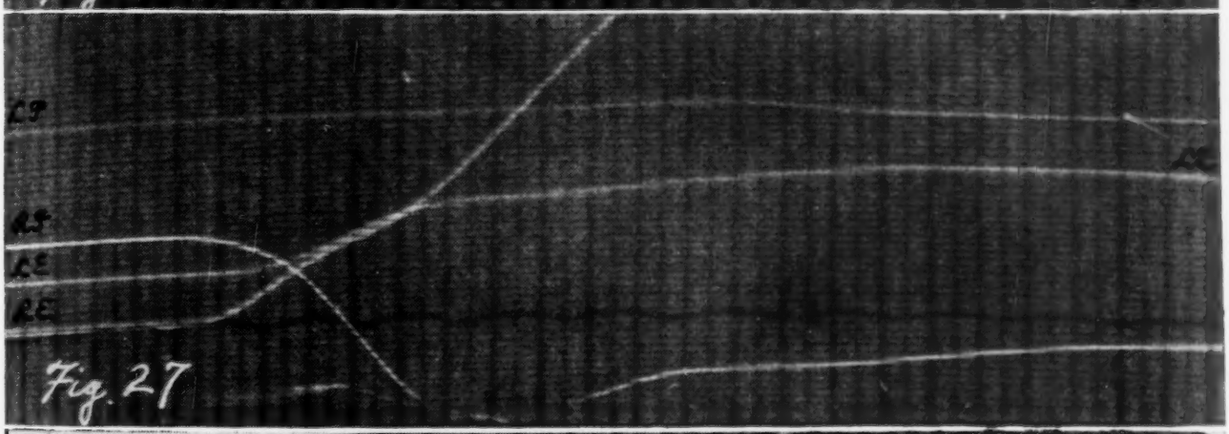


Fig. 27

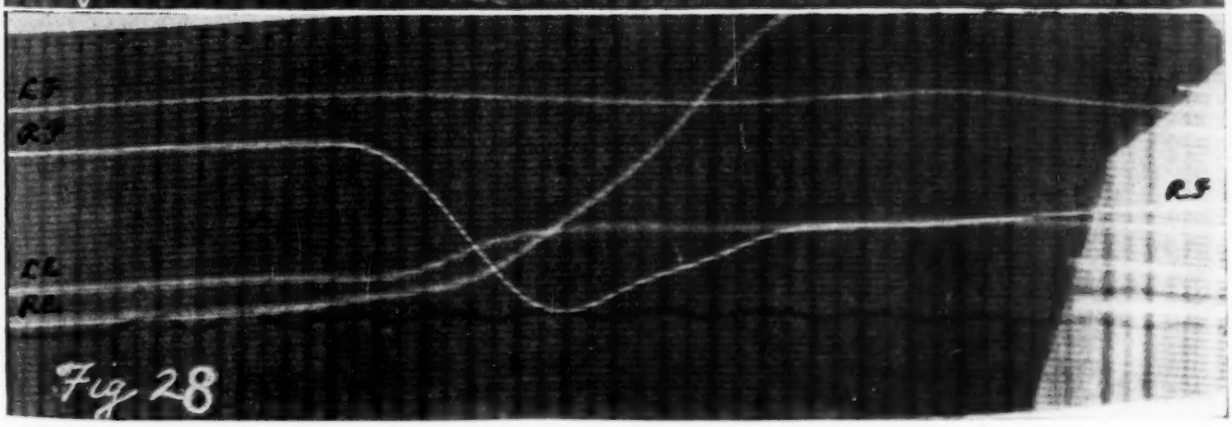
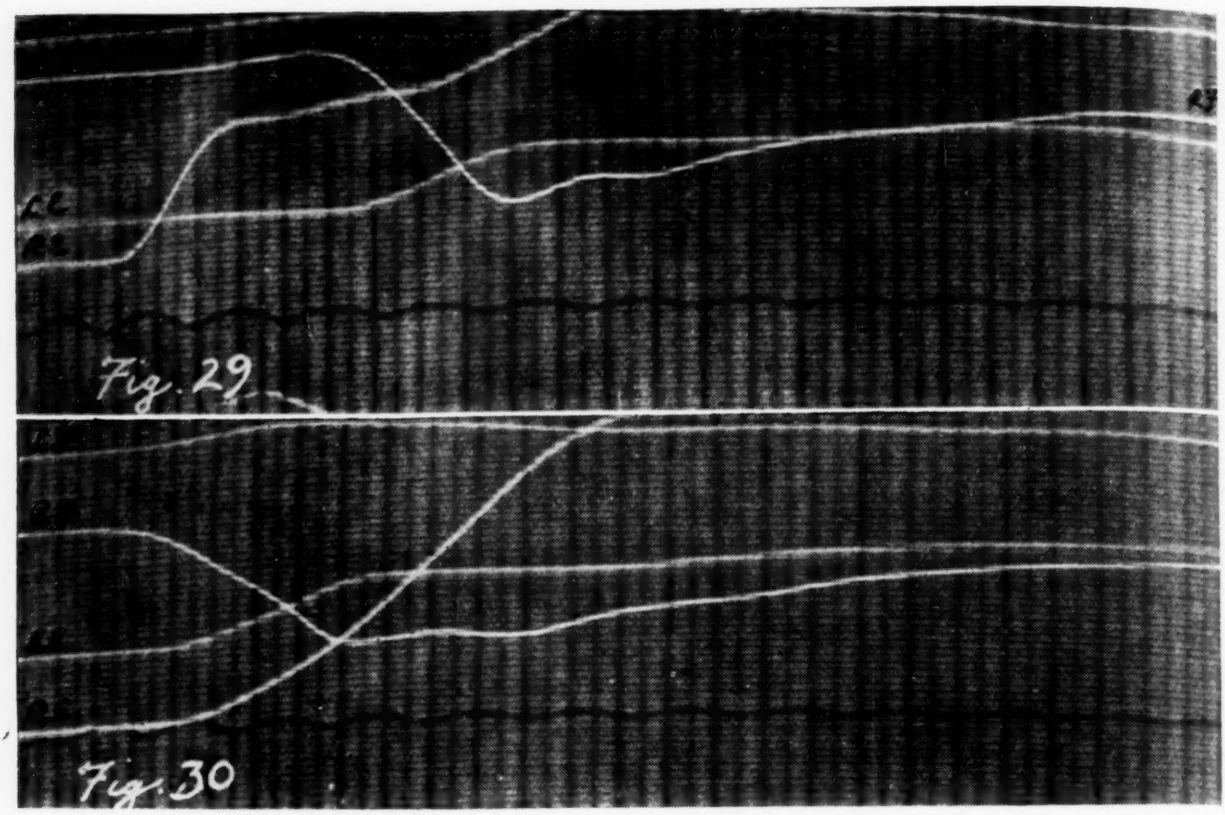


Fig. 28



records of all the general types are shown. Inhibition was considered definite if the right extensor curve rose smoothly, with no hump in it at a knee-jerk latency period after the blow. A knee-jerk latency was considered to be less than 56 sigma. Figure 22 fulfills these conditions. The slight hump in Fig. 23 came too late to be an uninhibited knee-jerk. A rise, preceding the main rise, that was as small as that in Fig. 26 was considered to be also an inhibited knee-jerk. No assumption was made in this class as to whether the inhibition was an effect of the back kick.

Cases listed between inhibition and lack of it include Figs. 28 and 30; and 27, in which the knee-jerk is definite, but the time relations are such that the back kick may exert some effect. Other doubtful cases where the rise of the leg obscures the determination of inhibition are 20 and 21. Records 12, 18, 19, 24, 25, and 29 were considered to show uninhibited knee-jerks, and it may be seen that while there is no definite dividing line between the types, only those cases in which inhibition is certain have been counted in the inhibited group.

In 18 and 29 there is no doubt of the presence of the knee-jerk, uninhibited in latency or height, because of the slope and also the early incidence of the back kick.

The knee-jerk is also evidenced in the rise of the left flexor in 24; and in 20, where, however, other factors may also enter in. Since this rise with the other kicks has been used to explain the action of the flexor, the latter can not be used to show that there is a strong knee-jerk present. The knee-jerk also brings about a rise in the first descent of the right flexor in 21, 29, and 30. This rise is not simultaneous with the much higher rise due to leg motion, which seems first definitely to appear in the flexor curve after its first dip.

Conclusions

1. Under the conditions of the experiment, Subject A practiced contracting the flexors of the percussed (right) leg and the extensors of the contralateral (left) leg in response to a buzzer signal at such a time interval before the knee-jerk blow that the voluntary kicks inhibited the knee-jerk. In most of the recorded cases of knee-jerks, tested without the buzzer or voluntary kicks after such practice, there is a change in the relative height and latency of the two jerks, consisting of a decrease in height and increase in latency of the right as compared with the left knee-jerk. This change lasts only through series of about 20 or 22 records, half from each leg; or approximately eleven minutes. That it is due to the voluntary practice is shown by comparison with similar practice and test series taken later, in which no buzzer was sounded and no voluntary kicks were made.

This change, once it has appeared, persists to a certain extent after practice series in which inhibition of the knee-jerk by the voluntary kicks is not definitely present in a majority of cases. Here the situation approaches that of the previous experiments (**2**), in which the voluntary leg motion definitely followed the knee-jerk because it was made in response to the blow itself, and in which a similar relative change in the knee-jerks was found.

The degree of inhibition could not be accurately determined, and records were only made of ¼ of the total voluntary

kick-knee-jerk combinations. For this reason the question is not settled whether the change is a matter of attention or of inhibition. There is the possibility that attention and inhibition are the same in this case, attention to the knee inhibiting the jerk.

2. An unexplained relative increase in the right knee-jerk of Subject A appears in all series following the beginning of voluntary practice, with the exception of the brief series which show the temporary decrease that is due to the voluntary practice.

3. No relative change in the knee-jerks of Subjects A and C is visible as a result of repeated elicitation in which many more of one knee-jerk than the other were elicited.

4. A downward motion in some part of the left leg of Subject A accompanies some right knee-jerks following the repeated elicitation of the right knee-jerk with few left knee-jerk elicitations. This motion appears to be due to a left flexor contraction and might be explained as an association of left flexion with right extension, following practice of left extension with right flexion and inhibited right extension. Its late appearance may be due to the removal of the immediate effects of the practice by repeated knee-jerk elicitation, or to a better distraction of attention in these series. In either of these cases, it might also be not an effect of the practice, but a normal response previously inhibited.

The lengthening of the right knee-jerk latency mentioned in Conclusion 1, if it be such, rather than a shortening of the left, is not assumed to be a weakening of the response. It may indicate that through repetition of the practice situation, the nervous elements involved in the knee-jerk include a larger proportion of those in the higher parts of the nervous system. There may or may not be inhibition of the lowest circuit by higher or voluntary nervous centers. If this change seen in the tests consisted only of a continuance of the 'set' toward voluntary response, or what would be in the usual conception a conditioning of the voluntary response to the knee-jerk blow as conditioned stimulus, evidence should be present in the records from the right flexor and left extensor in these tests; but such evidence is not present.

The early disappearance of the change does not indicate that it is negligible, for the disappearance is to be expected, since the testing of the knee-jerks itself consisted of further practice in the opposite habit, much better established through years of standing and walking. This change is in the direction of an unlearning of the right knee-jerk (or learning of the left). If it is assumed that a response which can be unlearned must to that extent have been learned in the first place, then the knee-jerk in the adult is not a reflex. It is hoped to continue the investigations by testing the knee-jerk in the infant from birth until walking is established, with a parallel record of the type of practice taken by the infant.

(Manuscript received, January 7, 1939)

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(For a more extended bibliography *cf.* reference 2.)

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VIBRATORY SENSITIVITY AS AFFECTED BY LOCAL ANESTHESIA

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INTRODUCTION

The present study was designed as an attack upon the problem concerning the mediation of vibratory sensitivity. Are vibratory sensations mediated by the bones and periosteum, some special end-organ for vibration, or are these feelings responsible to the same mechanism operating in the case of pressure sensations? All of these views have been postulated at one time or another in the history of the problem. The outstanding proponent of the first of these views is Egger (**5**), whereas Katz (**11**) is the name most frequently associated with the second, and von Frey (**6**) the investigator most usually coupled with the third viewpoint.

Various attempts have been made to settle the issue, the investigations coming from psychological and physiological laboratories as well as from clinical studies. One of the methods used in these investigations has been that of trying to isolate the vibratory sensations from other somesthetic experiences by means of anesthetization, or at least to determine the relative effect of drugs on pallesthetic and the other cutaneous sensations.

One of the earlier papers dealing with the effect of anesthesia on vibratory sensitivity was that of Goldscheider (**10**). He stated that in producing a light anesthesia by 'freezing,' the vibratory sensations evoked by a tuning fork were diminished, while with a deeper anesthesia, employing cocaine, the vibrations were sensed only vaguely or not at all when there was cautious application of the fork. However, in cocainizing a region over the tibia, Goldscheider found that

[1] The writer wishes to express his appreciation to Dr. Frank A. Geldard for his assistance in this work.

the vibrations could be perceived if a strong pressure gradient were used, although they were imperceptible when the fork was gently applied. From this investigation he concluded that vibratory sensations are mediated by cutaneous organs and also by the bones.

A year later Bing (1) reported some experimental observations concerning the effect of various drugs on vibratory sensitivity. The drugs used by him were carbon dioxide snow, cocaine, and ethyl chloride. His findings were that vibratory sensations were rarely materially diminished. From this he concluded that the drugs merely enhanced the transmission of the vibrations to the bones by causing an increased tissue density. For Bing, then, the bones were the active principle in the mediation of vibratory sensitivity.

Quite recently there have been studies by Cohen and Lindley (2) in which part of the work was devoted to the effect of skin anesthetization. In this they used ethyl chloride spray and novocaine. With the ethyl chloride they report a lowering of vibratory thresholds, whereas with the application of novocaine no change of vibratory sensitivity ensued. They, too, postulated the possibility of the ethyl chloride increasing the density of the tissue, thus creating a better conducting medium, and that the mechanism for the reception of vibratory sensations is located some place other than in the skin. This latter conclusion was based upon the findings with novocaine, for there was no diminution of the vibratory sensations produced with their apparatus, in spite of the fact that there was a removal of cutaneous sensitivity.

The work of Cummings (3), on the problem at hand, is the most recent study made at the present writing. In this investigation he reported the effect of cocainization on tactile and vibratory thresholds. The method used by him for drug application was electroendosmosis, a method developed by Rein (13). The same method of application was employed in the present study; hence the results should be more comparable, for in the other studies mentioned, excluding the application by a spray, as in the case of ethyl chloride, injection was used. The relative advantages of the two methods will be discussed later.

Cummings found that there was a consistent decrease in vibratory sensitivity occurring with cocainization, but it was very small in comparison to the very definite decrease in tactile sensitivity in the same area. In order to overcome the possible objection that tactile receptors outside the narcotized zone were participating, he anesthetized a larger area but found no concomitant increased loss of vibratory sensitivity. From these results he concluded that 'the peripheral mechanism of vibratory sensitivity is of a dual nature and involves both superficial and deep receptors.' Other observations made by Cummings will be discussed later in conjunction with the findings of the present investigation.

This brief summary of findings by no means characterizes the history of the general problem of vibratory sensitivity, but is meant merely as a basis for orientation with respect to the research done in determining the effect of anesthetization on the sensation in question. The literature of the general problem, including the effects of drugs, has been recently reviewed by Geldard (7).

Apparatus

The stimulus used throughout the work was a fine sewing needle 0.17 mm. in diameter and having a fairly sharp point. This was attached to the armature of an electromagnetic speaker unit (type B.B.L., distributed by the Wholesale Radio Service Company) which was activated by means of a General Radio 613-B beat-frequency oscillator. The impulses of the oscillator were led into a two stage push-pull amplifier. An L type attenuator, calibrated in decibels, was included in the circuit between the oscillator and the amplifier. This is, in part, the same apparatus described by Geldard and Gilmer (9). In all the experimentation a frequency of 256 cycles was used. The actual intensity control of the vibration was by means of a potentiometer which was calibrated in terms of amplitude of swing of the needle; thus a setting on the potentiometer could be directly translated into an amplitude in micra from a calibration curve.

To calibrate, a binocular microscope was rigidly trained on the vibrating needle. By means of a 'Strobotac' (General Radio 631-A) slightly mistuned to the frequency of the needle, thereby reducing the apparent motion to a very slow one, and having it arranged so as to illuminate the microscope field, the amplitude of swing could be measured by a micrometer eyepiece. Settings were made on the potentiometer and the corresponding amplitudes were recorded. These points were then plotted, potentiometer settings against amplitudes, in order to give the calibration curve to which previous reference was made.

The binocular microscope, with a magnification of about thirty diameters, was used in all the work for locating 'spots,' and by means of the eyepiece micrometer the skin gradient, produced by the stimulus needle, was kept constant.

The 'Strobotac' was used for lighting the field and a condensing lens was placed between the 'Strobotac' and the padded arm rest in which the arm was placed for all stimulations.

Preliminary Procedure.—Four square areas, 5 mm. on a side, were marked on the dorsal surface of the arm just above the wrist, by means of the Dallenbach injection technique (4). These four areas formed one large square area 10 mm on a side. After the skin had healed where the injections were made and there was no longer any inflammation present, photographs were taken of the individual areas. This was done as follows: the areas to be photographed were carefully freed of long hairs by cutting. Cutting was used rather than a depilatory or shaving since it was found that both of these irritated the skin and changed the characteristic skin markings. The arm was then placed in an arm rest to avoid movement. A photoflood lamp was so arranged as to have relatively the same position with respect to the arm as the light source in the actual experiments. The area to be photographed was then covered with green chalk dust, for it was found that this produced maximum contrast of the 'hills' of the skin with respect to the 'furrows.' The individual areas were then photographed with a Leica camera using the sliding focus, copy attachments. Best results were obtained with panchromatic film using an exposure of f. 6.3 at ⅛ second. The pictures were then enlarged twenty diameters and found entirely satisfactory for mapping purposes.[2] It was necessary to become completely familiar with the skin areas and to be able to locate rapidly on the photographic maps those 'spots' on the arm which were stimulated. This was accomplished in a relatively short time.

After thorough familiarity had developed with the skin areas and the sensations aroused by the stimulus, the actual experimentation was begun.

By means of the binocular microscope 'spots' were located and the vibratory thresholds taken. These 'spots' were located and marked on the photographic maps. The various areas were designated as N.W., S.W., N.E., and S.E. as they appeared in these positions in the microscope field. The mapped 'spots' were numbered and the threshold of the 'spot,' corresponding to its appropriate number, was recorded.

Thresholds were obtained by changing the intensity of the stimulus in small steps; between each step the arm was lowered from the needle. At first both ascending and descending series were taken but since there were little or no discrepancies between the two sets of readings, the descending measurements were discontinued.

A number of random 'spots' were chosen on the four areas and the vibratory thresholds taken. Before the stimulus needle was set into motion the arm was raised to it and the sensation evoked recorded. Upon vibratory stimulation not only were thresholds recorded but localization of the sensation was also noted.

After these measurements were made, the same 'spots' were stimulated a week later in order to check on the stability of the thresholds. It was found that they were constant since there was an inconsequential variability shown from one testing to the next.

The areas were then anesthetized by electroendosmosis of a cocaine solution. This method was employed to avoid wheals which are produced by intradermal injection, and further to control the size and position of the anesthetic area. Another advantage of this technique is that breaking of the skin, necessary with injection, is absent here.

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[2] The writer wishes to express his thanks to Mr. W. S. Verplanck who took and finished the pictures.

The apparatus used for the drug osmosis consisted of a 22.5 volt battery to which was attached, at the negative pole, a container of salt (NaCl) solution acting as the cathode. From the positive terminal there was a lead to a rheostat (15,000 ohms) which was in series with a second rheostat (20,000 ohms). Attached to this was a fixed resistance of 5,000 ohms acting as a protection for the milliammeter which was next in series. The milliammeter was connected to a switch from which there was a lead to an anode, consisting of a twenty-five cent piece fastened on the end of a short wooden rod.

A piece of filter paper, cut slightly larger than the anode, was saturated with a solution of the anesthetic and placed on the area to be anesthetized. The anode was held on the filter paper and the hand placed in the cathode solution. A current density of 0.33 M.A. per square cm was used, and depending upon the degree of anesthesia desired, variations could be made in both the quantity of solution used and the length of time employed. With this method a confined anesthetic area was obtained.

In this part of the experiment 0.5 cc of a five percent aqueous solution of cocaine hydrochloride was used and applied for twenty minutes. It was found that with this concentration of anesthetic and duration of application, the area was insensitive to prick, tested by stimulating with a needle, and to contact or pressure, as determined by using a von Frey hair at maximum intensity as the stimulus. Under these conditions the anesthetization was effective for about one half hour. Immediately after cocainization, the original 'spots' were retested for vibratory sensibility. Thresholds were recorded and localizations noted.

Table 1 summarizes the results found in this part of the experiment, showing 'spot' stimulated, threshold before and after cocainization, sensation elicited at the 'spot' with the needle not yet vibrating, and localization. Those stimulations from the stationary needle giving rise to either contact or pressure sensations are grouped together under the heading 'pressure,' while those stimulations evoking either a sensation of prick or pain are recorded under the heading 'prick.'

We can see from these data that, in general, those 'spots,' at which a report of 'pressure' or 'contact' was given, had lower vibratory thresholds than those at which 'prick,' 'pain,' or no sensation was reported. It can also be seen that the localization of the vibratory sensation was at or in the region of the stimulus needle when the area was not anesthetized.

By comparing the thresholds of the normal 'spots' with those obtained after narcotization, it is found that there is a marked rise in the stimulus limen for vibration. This stands in decided contrast to the results obtained by Cummings who found only a small increase in vibratory thresholds after drug application, even after the addition of adrenalin to give what he termed 'complete anesthesia.' Of course there is no

TABLE 1

VIBRATORY THRESHOLDS (μ)

Spot	Normal Area			Cocainized Area	
	Sensation from Stationary Needle				Localization*
	Insensitive	Pressure	Prick	Insensitive	
1	77			210	D
2	45			195	U
3		33		244	U
4		45		334	U
5		45		342	X
6		40		342	D
7			60	307	X
8			50	342	D
9		47		284	U
10		36		284	U
11	77			188	U
12			61	342	U
13		38		188	U
14		61		334	F
15	57			230	D
16	45			161	D
17		38		334	U
18		47		280	U
19	50			186	D
20		43		273	X
21		36		342	D
22		36		342	D
23	43			223	D
24		28		273	X
25	50			169	F
26	40			230	X
27			47	342	U
28		61		188	X
29	60			186	U
30		38		276	U
31		45		169	U
32		47		162	U
33			78	334	U
34		38		342	D
35	78			169	U
36		36		342	F
37		40		318	F
38		38		342	D
39		43		342	D
Averages	56.5	41.7	59.2	269	

* Vibratory sensation localized at or very close to 'spot' when normal area was stimulated.

D—very diffuse.

U—under side of arm.

F—fingers.

X—vague—upper arm near 'spot.'

method whereby a direct comparison of thresholds found in this study and those of Cummings can be made, since he used decibels of attenuation as a measure of amplitude and in the present work intensity is given in terms of amplitude of delivered stimulus in micra. There is also the discrepancy in reported localization. Cummings mentions the fact that there were some few peculiar localizations and explains them as 'probably due to placement of the stimulating rod directly over a tendon which conveyed the vibrations to a rich sensory field of a joint.' In the present study localizations of the vibratory sensations, when the vibrating needle was placed in the cocainized region, were *usually* vague and often on the under side of the arm. If observations were not carefully made, one might conclude that the sensation had a 'deep' origin.

In observing the needle vibrate in the cocainized region it was apparent that movement in this area was not like that in the normal skin. These observations were made by means of tuning the 'Strobotac' slightly out of phase with the frequency of the vibrating needle, thus slowing the apparent motion of the needle to any desired speed. With such a device the actual movement of the skin, caused by the repetitive stimulation of the vibrating needle, could be observed. In the normal skin area these disturbances travel in all directions from the stimulus, going out in ripples or waves. However, in the anesthetized area the whole region moved very much as a unit.

It was this observation which led to the next investigation concerning the transmission of these vibratory disturbances by the skin and a possible explanation of the divergencies of the results between the present study and that of Cummings.

It was now necessary to devise some means by which the spread of these waves, caused by the vibrating stimulus, could be measured. For this work a piezo-electric pick-up with a needle attached was used. This could be placed anywhere on the arm and the disturbances picked up if present. They were amplified by a General Radio 814-A amplifier, and heard through head phones as a tone of 256 cycles, since this

was the frequency at which both the needle and the skin were vibrating. Employing this technique, determinations of the extent of the transmission were then made on the dorsal side of the arm. This was done by systematically exploring the skin with the pick-up needle.

The distance away from the vibrating needle that a tone could be heard was limited by two factors: first, the amplification of the pick-up, and secondly, the extent of the disturbances.

The distances of transmission were determined when the stimulus needle was vibrating in the normal skin and in the cocainized region. In the normal area, when the stimulus needle was vibrating at threshold value, the extent of the disturbances, as measured by the presence of an audible tone at fixed amplification, rarely exceeded ten mm. from the point of origin; with increasing intensity there was an increased spread of these disturbances. In several cases tones were generated through the pick-up when it was placed on the palm of the hand, with the stimulus needle vibrating on the dorsal side of the arm just above the wrist. When the vibrating needle was placed in the anesthetized region and the transmission phenomena studied, it was found under these conditions, at equal amplitudes, that transmission was greater than when the vibrating needle was placed on normal skin. With the needle vibrating at threshold value in the cocainized area, the disturbances were found to exist as far away as the elbow. It will be remembered that the anesthetic zone vibrated as a unit; this would account for the greater spread effect at the same intensity of stimulation. In this case we have essentially a vibrating stimulus with a large contactor and are no longer dealing with the needle as a stimulus. In this area, after anesthetization, the smooth muscle elements are constricted. This is shown by the presence of 'goose flesh.' The area itself has a 'solid' appearance lacking in normal skin.

Another phenomenon noted, when the pick-up needle was placed in the normal skin area and the stimulus needle was vibrating at a subthreshold level in the anesthetized region,

was that with the application of the pick-up needle sensations of vibration were often elicited. A careful study of this was then made.

The procedure followed was to anesthetize an area on the dorsal side of the forearm and apply the vibrating needle to this region at a subliminal level. The observer was instructed to report any sensation elicited when stimulated with the pick-up needle. The anesthetic used in this case was procaine and it was applied by the same osmotic method to which previous reference has been made. It was found that reports of 'prick,' 'contact,' 'pressure,' and 'vibration' were given. Very often reports of 'vibration' were given accompanied [3] by reports of 'contact' or 'pressure'; never was one accompanied by a report of 'prick.' When a report of 'vibration' was given localization was reported as being at the pick-up needle. If the disturbances were traveling the distance between the stimulus needle and the pick-up needle, as determined by the presence of an audible tone, reports of 'pressure' and 'contact,' upon application of the pick-up needle, were also accompanied by reports of 'vibration.' [4]

The fact that vibratory sensations could be elicited under these conditions, may be explained by assuming a reversal of the ordinary situation, that is, instead of the needle vibrating against the skin, the skin was actually vibrating against the needle. This would account for the vague localizations of vibratory sensations when the stimulus needle was vibrating in the anesthetized region.

It will be remembered that many of the localizations were on the under side of the arm when threshold was reached. This would now be explained by assuming that the skin in this region was vibrating against the arm rest and, due to the many contacting points afforded here, the localization would naturally be diffuse.

However, an explanation of the greater spread of vibratory disturbances in tissue covering bony areas would not be sup-

[3] The reports of 'contact' or 'pressure' always preceded reports of 'vibration'; the two were not felt simultaneously.

[4] The disturbances were undoubtedly traveling farther than was shown by the pick-up needle. However, they were probably too small in amplitude to give either an audible tone or act as a stimulus for the vibratory sensation.

plied by this interpretation. Consequently an exploratory type of experiment was conducted to determine if there were any explanation of this other than that presented by Egger (**5**), in which he assumed the periosteum and the bones to be both the conductors and receptors for vibratory sensations.

It was hypothesized that the skin acts very much as a diaphragm for these vibratory movements. If such is the case then the more tightly the skin is stretched the greater the enhancement of vibration for the frequency in question. This hypothesis was tested by determining the extent of transmission, again by means of the piezoelectric pick-up, in normal skin and in the same area held taut with adhesive tape. It was found that under the conditions of stretched skin the vibratory disturbances traveled much greater distances than in the same skin in the normal state. This would perhaps account for the greater spread of vibration over bony regions, for here the skin is more tightly stretched and there is not as much fatty tissue present to create a damping effect. This notion is in harmony with the view presented by von Frey (**6**). It is further substantiated by a report made by Minor (**12**), in which he states that in cases of broken bones and even in fractures where there was a complete break, resulting in the two ends being separated, there was no diminution of vibratory sensibility when a tuning fork was applied to the distal end of the limb in question.

Another point may be made which would tend to disprove the view that the bones or the periosteum are the receptors for vibration. In the anesthetization experiments here conducted, it was found that the vibratory thresholds rose tremendously as compared with those on the same 'spots' in normal skin. This method of application of the anesthetic gives a rather superficial anesthesia, certainly not as deep as the periosteal tissue lies. Hence, if this were the case it would seem reasonable to assume that this tissue was unaffected. Consequently, why then should there be a rise in thresholds if the receptor for the sensation was unaltered?

In the investigation using procaine as the anesthetic it was noted that on recovery of sensitivity of the area prick returned

first, followed by contact, and later pressure. It was thought advisable to study the recovery of vibratory sensibility in a procainized area. The following experiment was therefore performed.

The original areas were anesthetized with procaine, using a 0.5 cc of a two percent solution for twenty minutes. Ten 'spots' in area S.W. were chosen on which to observe the recovery of sensations of vibration. These ten 'spots' were stimulated in random order and the time noted. Each time the stimulus needle was applied to a 'spot' a report was made of the sensation elicited before the needle was set into vibration. Vibratory thresholds were then taken. However, unlike the original determinations, here the arm was not removed from the needle between steps in potentiometer settings but the amplitude was steadily increased. This was done in order to eliminate the delay between stimulus presentations.

It is interesting to note that although this work on the recovery of vibratory sensibility was done several months after the original testing of vibratory thresholds there was surprisingly little difference in the thresholds originally obtained and those obtained after complete recovery. It should be indicated that the photographic maps were entirely adequate at this much later date for relocating the 'spots' in the area studied.

Table 2 summarizes the results of this experiment.

In order to obtain a curve of recovery two 'spots,' No. 11 and No. 13, in area S.W. were chosen. In this way more stimulations of each individual 'spot' could be had during the effective duration of the anesthesia. One of the 'spots' was pressure sensitive and the other insensitive as determined before anesthetization. This selection was made arbitrarily by defining a pressure sensitive spot as one which on stimulation would cause a response of either 'contact' or 'pressure,' using the smallest visible skin gradient as seen through the binocular microscope. The criterion of 'insensitivity' was the absence of any response upon stimulation of a 'spot' when the skin gradient extended completely across the

TABLE 2

Area: S.W.—procainized
Frequency: 256 cycles.

Spot	Thresh. (μ)	Time	Rep't.
1	284	2:12	In.
3	342	2:18	In.
4	334	2:15	In.
6	342	2:24	Pr.
7	208	2:23	Pr.
10	177	2:26	Pr.
11	342	2:17	In.
13	177	2:19	Pr.
14	342	2:14	In.
16	334	2:21	Pr.
1	136	2:28	Con.
3	69	2:30	Con.
4	138	2:33	Pr.
6	64	2:35	Pr.
7	77	2:37	Con.
10	75	2:40	Pr.
11	50	2:39	Con.
13	80	2:42	In.
14	223	2:31	In.
16	84	2:44	Pr.
1	76	2:50	Con.
3	38	2:58	P.
4	69	2:51	Con.
6	50	2:49	Con.
7	47	2:55	In.
10	61	3:00	In.
11	38	3:01	P.
13	80	2:56	In.
14	74	2:46	In.
16	80	2:53	In.
1	45	3:05	P.
3	45	3:07	P.

Rep't. indicates report given before needle was vibrating.

Con.—contact
P. —pressure
Pr. —prick
In. —insensitive

microscope field.[5] These were the same criteria used by Geldard (**8**).

The area was again procainized using 0.5 cc of a two percent solution for thirty minutes. The procedure was exactly as before with the exception that the two 'spots' were stimulated alternately.

The same experiment was repeated using two other 'spots.' Six months later this same procedure was followed, using another subject. All results are in substantial agreement. Fig. I presents graphically the results of the first recovery series mentioned.

[5] The extent of the deformation was 8 mm in diam.

By an examination of the graph it can be seen that with the return of prick sensitivity there was little change in vibratory thresholds. With the advent of the reports of 'contact,' however, the vibratory thresholds declined immediately. These thresholds continued to decrease toward normal and as soon as the area became pressure sensitive it can be noted that the 'insensitive spot' reached normal threshold value, whereas the 'sensitive spot' took a small dip in threshold and very shortly thereafter was also normal. Throughout the recovery of vibratory sensitivity the 'sensitive spot' was more rapid in

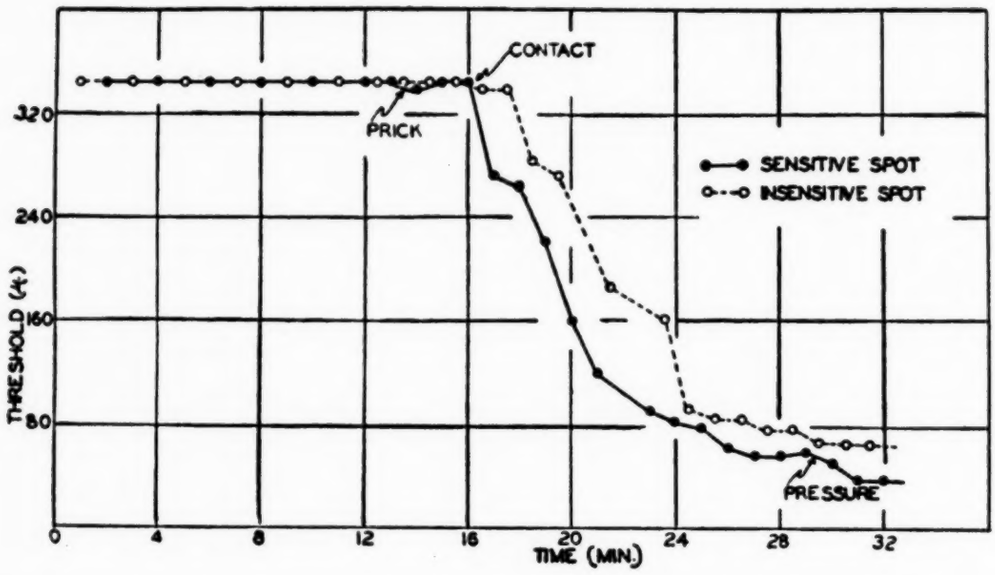

FIG. 1. Curves of recovery for 'pressure sensitive spot' No. 11 and 'insensitive spot' No. 13.

regaining normalcy than was the 'insensitive spot.' With the return of contact sensitivity to the area studied the localizations of the vibratory sensations were more accurate and with the return of pressure the localizations were completely normal.

DISCUSSION

Combining the above observations we may have a basis of interpretation for the results obtained in the various experiments using anesthesia.

It will be remembered that Goldscheider found a diminution of the vibratory sensations when the fork was cautiously

applied to the narcotized area. However, using a stronger pressure gradient no decreased sensitivity was found. If the 'cautious application' resulted in a very minute skin gradient then, in the light of the above findings, we might expect that there was little opportunity for spread of these disturbances to a normal region, whereas with a stronger pressure gradient the spread would be sufficient to overcome any local reductions in sensitivity. This would be especially true since not only would the strong application of the fork stretch the skin, thus enhancing conduction, but also with such a large stimulating area as that afforded by the foot of the fork, the mechanical possibility of the tuning fork moving the wheal as a unit would be greatly increased.

The findings here agree with those of Bing inasmuch as he stated that the anesthetized tissue became more dense, but in view of the results obtained in the present study there seems to be no reason for postulating a conduction of the disturbances to the bones. The explanation of the lack of change of sensitivity as given in the case of Goldscheider's work might very well apply to Bing's research.

Similarly in the case of Cohen and Lindley's study; their stimulus was of relatively large dimensions, 7.07 mm. in diameter, and hence would be mechanically capable of vibrating the narcotized area. It would even be more readily conceivable since in this study, as in the two above mentioned studies, there was a wheal to contend with, which would act more like a unit than an area anesthetized by the osmotic method.

In none of the previously cited investigations was localization dealt with. In Cummings' work it was only briefly mentioned. Where a tuning fork is used as a stimulus one would not expect localization to be remarked, for normally the sensation elicited under these conditions is hardly to be called discrete. On the other hand Cummings' stimulus had a much smaller contacting surface, being one mm. in diameter. Even so this diameter was at least ten times that of the stimulus used in the experiments reported here. The stimulus used by Cummings might very well have been large enough

to require very little added amplitude to set the whole cocainized region into vibration. The pressure gradients used by him were also probably greater than those used in the present investigation, since his criterion was a just visible indentation of the skin observed by the unaided eye. This too would account for the small drop in sensitivity as contrasted to the marked change found in the present investigation.

Cummings' explanation of the few cases of peculiar localizations as being due to conduction of the vibrations to the rich sensory field of a joint would no longer seem necessary in view of the results found with the piezo-electric pick-up, previously reported in this paper. It is unfortunate that the two studies are not more comparable due to the fact of differences in size of stimulator and possible differences in skin gradient used. It is undoubtedly due to these differences that 'peculiar localizations' were not more frequently noted in the Cummings research. Had there been a more adequate study of them there might possibly have been some modification of his conclusions. These divergencies probably explain the discrepancy in results between the two investigations.

From all of the work reported here it would seem that the third of the three views stated in the introduction of this paper, namely that vibratory feelings are responsible to the same mechanism as that operating in the case of pressure sensations, received the most support.

First, there was the relationship of the lower vibratory thresholds at the contact and pressure sensitive 'spots.' Second, it was found that with a cutaneous anesthesia vibratory thresholds rose markedly. Third, vibratory sensations could be elicited at the pick-up needle at contact and pressure sensitive 'spots,' whereas they never occurred at prick sensitive 'spots.' Fourth, vibratory feelings and normal localization returned at approximately the same time as did contact upon recovery from procainization. And finally, in recovery from anesthetization vibratory thresholds did not return to normal until pressure sensitivity had shown complete recovery.

Summary

Using an apparatus capable of delivering well controlled vibratory stimulation, measurements were made on a number of 'spots' on the dorsal side of the arm just above the wrist to ascertain stimulus thresholds for vibration. The region was then cocainized by electroendosmosis and the same 'spots' retested for vibratory thresholds. A marked rise was found in thresholds at the second testing.

Recovery of sensitivity was followed after procainization and it was found that prick returned first. However, there was no accompanying change in vibratory sensitivity. With the return of contact a marked drop was noted in the vibratory thresholds and they returned to normal with the recovery of pressure sensitivity.

Transmission phenomena were studied by means of a piezo-electric pick-up, and it was determined that the disturbances, caused by the vibratory stimulation, traveled relatively great distances. Observations were also made which showed that these distances could be increased by stretching the skin with adhesive tape.

Stimulating in the anesthetic zone, vibratory sensations were elicited when the pick-up needle was placed on a pressure or contact sensitive 'spot' in an external normal region. The phenomenon was assumed to be a basis of explanation for unusual localizations.

All the results seem to support the view that vibratory sensations are mediated by the same mechanism mediating pressure sensations.

(Manuscript received January 9, 1939)

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METABOLISM AND PULSE RATE AS RELATED TO READING UNDER HIGH AND LOW LEVELS OF ILLUMINATION *

BY ROSS A. McFARLAND, CHARLES A. KNEHR AND CONRAD BERENS

A distinction is being made in recent literature between the older *science of vision* and the newer *science of seeing* (**6**). The *science of vision* is said to be limited in its practical usefulness in prescribing hygienic light intensities for such a task as reading, largely because it can measure only visual thresholds, and in addition it has not taken into consideration the biologically complex individual of which vision is a psychophysiologically functional part. The newer *science of seeing* is not limited to thresholds or to the study of 'barely' seeing. It has an extended viewpoint 'to seeing as a human activity which involves energy, muscles, nerves, organs, body and mind in addition to the two eyes and the visual sense' (**9**). Furthermore, it is interested essentially in producing hygienic lighting which will allow a given task (such as reading) to be performed with the minimum cost to the individual.

It is generally recognized that greater stresses are being placed on the visual sense in modern education and industry. More reliable and objective criteria than merely subjective sensations of ocular discomfort or eyestrain are needed for determining levels of illumination which will provide easy visibility and comfort and which will prevent ocular defects. Researches such as König's classic experiments (**4**) and others (**2**, **11**, **12**, **13**, **14**) form the basis for recommendations of about 10 to 15 footcandles (with good contrast and diffused light) for eyes with normal refractive and muscular conditions. These studies are representative of what Luckiesh and Moss

* From The Fatigue Laboratory, Harvard University, Boston, Mass. This study was approved in principle by the American Committee on Optics and Visual Physiology and the Association for Research in Ophthalmology. The General Electric Company very kindly contributed funds for the investigation through the Ophthalmological Foundation, Inc., New York, N. Y.

(6) have termed the *science of vision*, which is concerned primarily with alterations in visual functions from the limited point of view of the effect of illumination on the eyes alone.

The researches which form the basis for the new *science of seeing* show alterations in certain physiological processes not primarily visual with changes in illumination. Luckiesh and Moss have reported, for example, that nervous muscular tension, as indicated by changes of pressure applied by the finger tips, increases about 50 percent when illumination in a reading task is reduced from 100 footcandles to 1 footcandle (5). In another study (7), pulse rate decreases about 10 percent during reading for 1 hour at 1 footcandle, whereas the decrease is approximately 2 percent at 100 footcandles. Blinking rate, which seems to be related to mental tension (8, p. 223), increases 70 percent while reading at 1 footcandle, whereas the increase is about 8 percent at 100 footcandles. These psycho-physiological changes have become the criteria for recommendations (6, p. 5) (again assuming that we have an ideal situation regarding quality of lighting, contrast, control of glare, etc.) of from 100 to several hundred footcandles (intensities approaching daylight levels) for prolonged reading.

In this study we sought evidence of a relation between the level of illumination while reading and basal oxygen consumption and pulse rate. It was assumed that, if such a relation could be observed, the metabolic changes might furnish additional and more fundamental criteria for determining adequate levels of illumination for tasks such as reading. The increase in muscular tonus, and other physiological variations observed while reading under poor illumination mentioned above, appeared to be large enough to be reflected in the metabolism of the individual. It has been stated that these changes are indicative of the cost of reading in dim light, with the implication that it may endanger the health and welfare of the individual.

Procedure.—The research was carried out in the air-conditioned room in the Fatigue Laboratory, with temperature and humidity maintained at 70° ± 2° F. and 45 percent ± 5 percent relative humidity. The semi-indirect illumination was solely artificial; no light entered the room from without. Sound proofing insured relative freedom of the subject from outside distractions.

The procedure for the 21 subjects was as follows. Each subject came to the test room in the morning without breakfast and sat quietly throughout the experiment. After resting for 30 minutes, the pulse rate was taken and the subject was then connected to the gasometer with a minimum of disturbance. He then read for 2 hours, the first hour at 50 footcandles and the second at 1 footcandle. During the 2 hours, the expired air was collected in the gasometer for 16 periods of 5 minutes each and the pulse rate counted for a full minute every 15 minutes. Samples of expired air for alternate 5 minute periods were withdrawn from the Tissot and analyzed on the standard Haldane apparatus. To avoid disturbance, the mouthpiece and nose-clip remained in place throughout the entire 2 hour period. On a succeeding morning, the experiment was repeated with the order of illumination reversed. Control runs indicated that there were no significant effects on metabolism or pulse rate which might be attributed directly to the experimental conditions, that is, to the mouthpiece or nose-clip.

The first 11 men whose tests were completed read 8 point type on paper with an approximate reflection factor of 85 percent under both brightness levels. For the remaining 10 men, the reading was made more difficult at 1 footcandle by the introduction of 6 point type on a paper with reflection factor of about 60 percent, and relatively easier at 50 footcandles with 12 point type on paper of about 80 percent reflection factor. Since the difficulty of the task was presumably altered, the data for this group was handled separately from those of the preceding group. We shall designate them Group A and Group B respectively.

Results.—The values for oxygen consumption in cc per minute are shown in Tables I and II for Groups A and B respectively. These data are shown graphically in Fig. 1 both for 15 minute intervals and for the total averages for each hour. Inspection of the total averages reveals that, on

TABLE I

OXYGEN CONSUMPTION IN CC/MIN FOR 11 SUBJECTS DURING 2 HOURS' READING 8 POINT TYPE AT 1 AND 50 FOOTCANDLES

Group A

	First hour—1 footcandle				Second hour—50 footcandles			
	15 minutes	30 minutes	45 minutes	60 minutes	15 minutes	30 minutes	45 minutes	60 minutes
Average (cc/min)	232	242	245	245	243	253	245	247
Δ percent.....	—	4.3	5.6	5.6	4.7	9.1	5.6	6.5
S.D.....	39	37	58	50	29	50	45	44
Total average...				*241*				*247*
	First hour—50 footcandles				Second hour—1 footcandle			
Average (cc/min)	241	247	242	239	250	254	255	238
Δ percent.....	—	2.5	0.4	−0.8	3.8	5.4	5.8	−1.2
S.D.....	30	42	37	38	30	37	24	35
Total average...				*242*				*249*

TABLE II

OXYGEN CONSUMPTION IN CC/MIN FOR 10 SUBJECTS DURING 2 HOURS' READING 6 POINT TYPE AT 1 FOOTCANDLE AND 12 POINT TYPE AT 50 FOOTCANDLES

Group B

	First hour 1 footcandle—6 point				Second hour 50 footcandles—12 point			
	15 minutes	30 minutes	45 minutes	60 minutes	15 minutes	30 minutes	45 minutes	60 minutes
Average (cc/min)	248	252	262	265	259	263	261	260
Δ percent	—	1.6	5.6	6.8	4.4	6.0	5.2	4.8
S.D.	41	37	24	28	38	28	40	37
Total average				*257*				*261*
	First hour 50 footcandles—12 point				Second hour 1 footcandle—6 point			
Average (cc/min)	255	261	265	263	258	253	264	258
Δ percent	—	2.4	3.9	3.1	1.2	−0.8	3.5	1.2
S.D.	33	36	39	25	34	27	30	30
Total average				*261*				*258*

the whole, oxygen consumption by Group A was approximately 3 percent higher for the second hour than for the first, whether low or high illumination prevailed. The total average metabolism for Group B was higher for reading at the 50 footcandle level by approximately 1.5 percent as compared to the 1 footcandle level. Though none of these figures approach statistical significance, the tendency for Group B would seem to be exactly in the opposite direction from what would be expected if reading at 50 footcandles were a less severe task compared with reading at 1 footcandle.

The averages for 15 minute intervals show the same independence of the illumination level. It can be readily seen from Fig. 1 that oxygen consumption is neither consistently higher nor consistently lower for reading at a low illumination as compared with a high level. The tendency for a rise (not significant) in metabolic rate during the first 30 minutes of the experiment may be noted.

It is obvious, then, that there is no consistent trend in the data which can be taken as indicative of a relation between the level of illumination (or difficulty of the task) and oxygen

consumption of the subjects taken as a group. The above changes must be ascribed to experimental error and to individual variations. The results for the individual subjects showed a range of as much as 10 percent, and no consistency was shown by the various subjects either in the direction or the amount of change in oxygen consumption. The standard

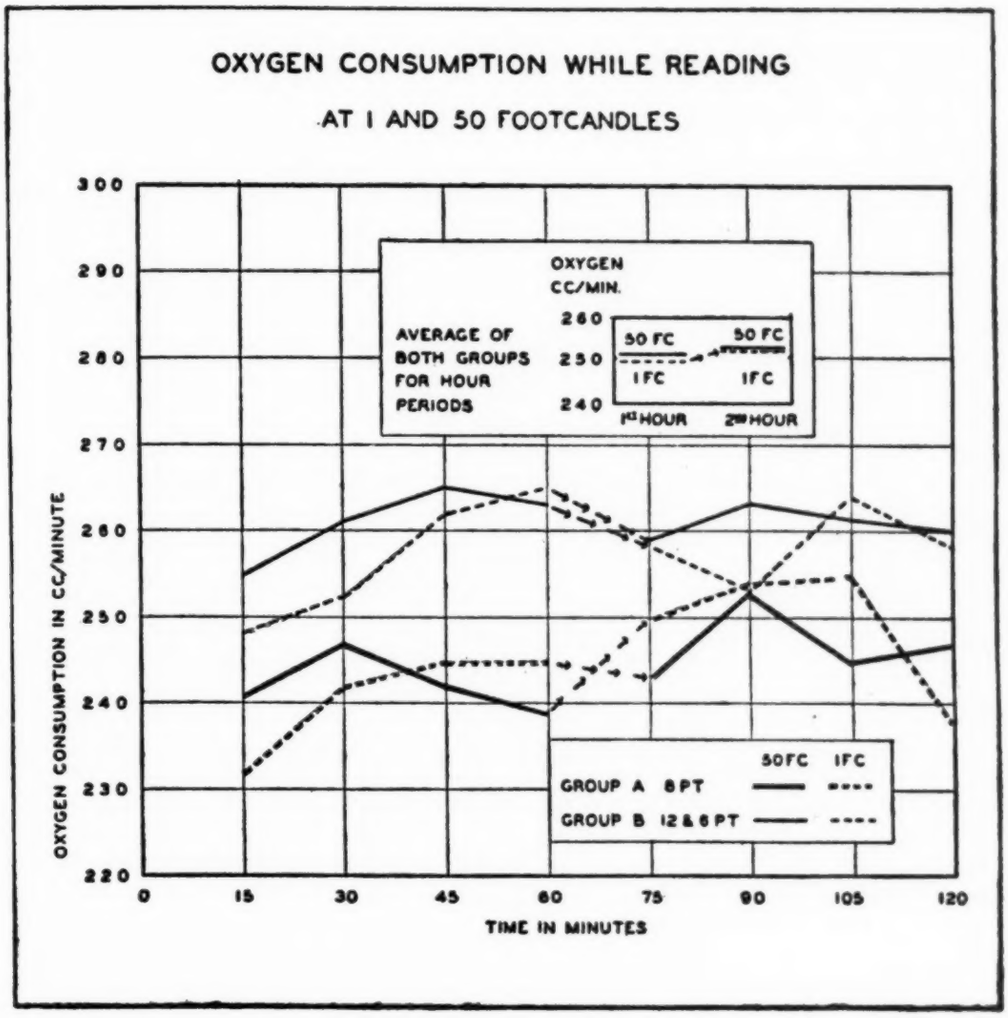

FIG. 1

deviations shown in the tables were obviously large, due to individual differences in metabolic rate.

The averages of the pulse rates for Groups A and B are tabulated in Tables III and IV. These data are shown graphically in Fig. 2. If the data are considered as a whole, the decrease in 5 percent while reading for 1 hour at 1 footcandle, and 6 percent while reading at 50 footcandles. It is

TABLE III

AVERAGE PULSE RATES OF 10 SUBJECTS DURING TWO HOURS' READING OF 8 POINT TYPE AT ILLUMINATIONS OF 1 AND 50 FOOTCANDLES

Group A

		First hour—1 footcandle				Second hour—50 footcandles			
	Control	15 minutes	30 minutes	45 minutes	60 minutes	15 minutes	30 minutes	45 minutes	60 minutes
Average	65.0	63.9	62.9	63.3	62.2	63.3	62.0	62.4	60.3
Δ percent	—	−1.7	−3.2	−2.6	−4.3	−2.6	−4.6	−4.0	−7.2
S.D.	7.5	7.2	6.6	6.1	6.4	5.7	7.6	6.2	6.3
		First hour—50 footcandles				Second hour—1 footcandle			
Average	67.1	68.1	66.8	66.0	66.3	64.7	63.4	62.8	64.0
Δ percent	—	1.6	−0.3	−1.5	−1.0	−3.3	−5.4	−6.3	−4.5
S.D.	8.0	8.3	8.3	7.9	8.5	9.1	8.6	8.9	8.3

TABLE IV

AVERAGE PULSE RATES OF 10 SUBJECTS DURING 2 HOURS READING 6 POINT TYPE AT 1 FOOTCANDLE AND 12 POINT TYPE AT 50 FOOTCANDLES

Group B

		First hour 1 footcandle—6 point				Second hour 50 footcandles—12 point			
	Control	15 minutes	30 minutes	45 minutes	60 minutes	15 minutes	30 minutes	45 minutes	60 minutes
Average	67.7	65.9	66.8	65.8	65.5	63.9	63.8	63.4	63.1
Δ percent	—	−2.7	−1.3	−2.8	−3.3	−5.6	−5.8	−6.4	−6.8
S.D.	9.6	9.0	9.8	9.7	9.7	9.6	10.2	10.0	10.0
		First hour 50 footcandles—12 point				Second hour 1 footcandle—6 point			
Average	68.2	67.3	66.3	65.8	64.7	62.2	62.2	62.5	61.0
Δ percent	—	−1.3	−2.8	−3.5	−5.1	−8.8	−8.8	−8.4	−10.5
S.D.	7.7	7.6	7.4	7.4	7.5	7.3	8.3	7.1	7.4

obvious that there is no direct relationship between the pulse rate while reading under the different levels of illumination in this experiment for either Group A or B. The general trend of the pulse rate is downward, as shown in Fig. 2, irrespective of the level of the illumination.

Discussion.—There are several possible explanations for the above results relating to oxygen consumption under the different levels of illumination. Perhaps the total bodily metabolism was not affected by changing the level of illumination while reading, in which case no consistent trend would be found in the data. It may be that reading under poor illumination does not involve a great enough stress on the muscles or other organs of the body to alter significantly the metabolic

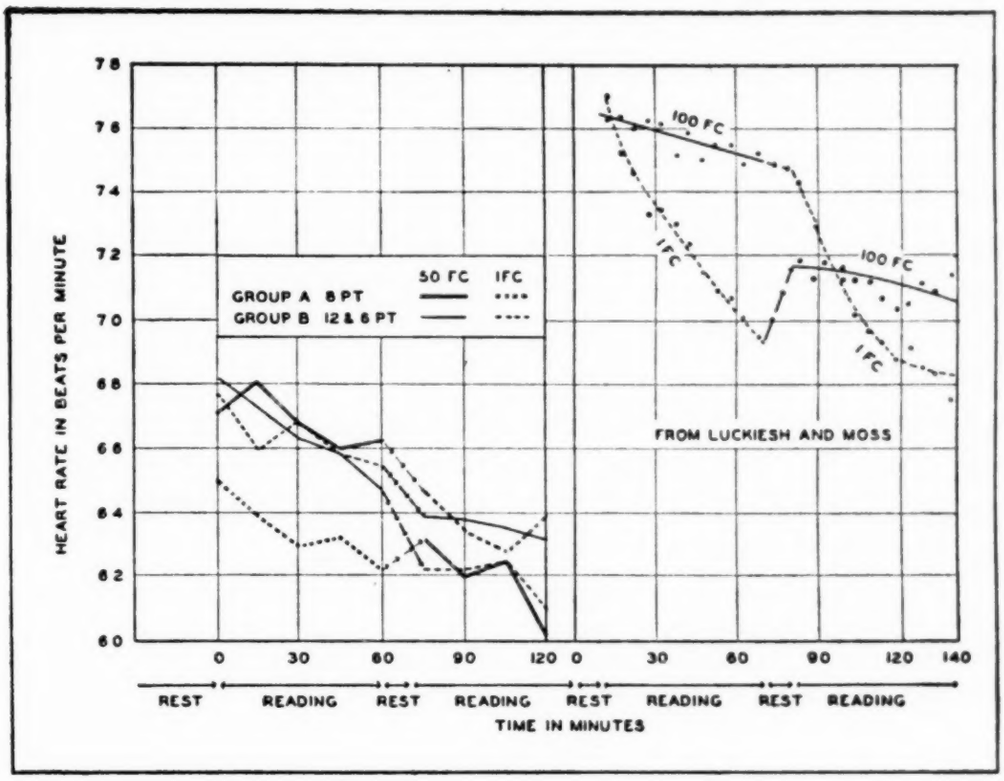

FIG. 2

rate. On the other hand, there may have been changes in oxygen consumption, but the technique may not have been refined enough to detect the minor variations; they may have been masked by minor bodily movements. In any case, if metabolism is altered by the difficulty of reading at 1 foot-candle, the change is small; and it is safe to conclude that in our subjects there is no relation between the oxygen consumption of the body as a whole and the level of illumination on the reading material.

Since we observed no significant changes in metabolism while reading under low compared with high illumination, it was not surprising that the variations in pulse rate in relation to illumination were not significant. If the metabolism had increased because of an increase in muscular tonus (which Luckiesh and Moss have observed under poor illumination), one might have expected, on the basis of known physiological principles, an increase in pulse rate. However, Luckiesh and Moss have reported a significant drop in pulse rate under poor illumination. They state that their research "establishes as a fact, that a close correlation exists between the pulse or the heart rate, and the intensity of illumination under which critical visual work is performed" (7, p. 17). In a further discussion (8, p. 222) the authors indicate that the drop in pulse rate (10 percent) while reading for 1 hour at 1 footcandle cannot be casually explained. They state, for example, that "these visual tasks are abnormally severe and unnatural in character and it follows that the heart and the vascular system in man have not been developed through evolutionary processes to adequately compensate for the physiological changes these tasks produce." In general, they consider these changes to be deleterious to the organism.

The variations in pulse rate reported by Luckiesh and Moss compared with the data obtained in this experiment are shown in Fig. 2. The differences can be explained in part on the basis of the different experimental procedures. The higher initial pulse rate (approximately 10 beats per minute) in their study is due to the fact that their subjects were not fasting. In an attempt to measure the influence of the illumination on the metabolism and pulse rate while reading, our subjects came to the laboratory in the morning without breakfast so as to begin the experiment in the post-absorptive state. In both sets of data there was a general tendency for the pulse rate to decrease. This might also be expected if the subjects remained at rest during a two-hour period with no more of a strenuous task than reading. However, the increase in pulse rate of 2 beats per minute when the illumination was changed from 1 to 100 footcandles and the drop of 7 beats per minute

during the second hour with poor illumination (cf. Fig. 2) in the Luckiesh and Moss data remains unexplained.

One possible explanation for these variations in pulse rate in relation to high and low illumination may be related to the effects of temperature variations on the heart rate. The air-conditioned room used in our investigation kept temperature constant and minimized the effects of radiant heat from the electrical units. Grollman (**3**) has shown that the pulse rate in rest increases when the environmental temperature rises above 68° F. In describing the processes involved, Grollman states (*op. cit.*, p. 124) that "the heart responds by pumping an additional amount of blood equal to that which is required to maintain the circulation through the cutaneous areas in which vaso-dilation occurs as a result of the heat." This effect is also observable due to radiant heat when the air temperature is constant.

Furthermore, in discussing the relation of oxygen consumption to temperature changes, Grollman (*op. cit.*) states that with decreasing temperatures below 68° F. there is a rapid increase in oxygen consumption. "Even when muscular movements are not evident, one must consider the increased muscular activities due to an increase in tonus of the general musculature at lower temperatures." (Grollman, *op. cit.*)[1] It is possible, therefore, that uncontrolled environmental temperature may account in part for the changes in tension and heart rate observed in the Luckiesh and Moss experiment. Another possible explanation may be related to the oculo-cardiac reflex. It is well known that this reflex tends to slow the pulse rate. Reading under conditions of low illumination may evoke this reflex in some manner. Even if environmental temperature or the oculo-cardiac reflex had not been involved, it is difficult to interpret the findings of Luckiesh and Moss in regard to the increase in muscular tension and the decrease in pulse rate while reading under low illumination. It is difficult

[1] It should be recognized, however, that not all investigators have found such striking results as those reported by Grollman. For example, HARDY, J. D., MILHORAT, A. T., and DUBOIS, E. F. (*J. of Nutrition*, 1938, **16**, 477) found that nude subjects in a Sage colorimeter at temperatures which produce chills in 2 to 3 hours (71.6° to 77° F.) showed no change in basal metabolism until 10 to 15 minutes before the onset of chills.

to understand how in their experiments it was possible to have a decreased pulse rate and an increase of 50 percent in a measure which they believe gives an index to muscular tension.

The results of our investigation are in general agreement with those obtained by Benedict in a study of the metabolic cost of mental effort (1). In carefully controlled experiments he observed that during an hour of intense mental effort, consisting chiefly in the multiplication of 2-digit figures, there was no significant increase in oxygen consumption (4 percent) and heat production. Since the total amount of tissue involved in the organs of vision is so small, one would hardly expect the oxygen consumed while reading to be significantly greater than the amount consumed during intense mental effort. This would follow, of course, only in so far as the material was not emotionally exciting or a considerable amount of the musculature of the body was not involved during prolonged or boring reading periods. We suggest, therefore, that studies dealing with the attempt to establish reliable criteria for adequate illumination for reading should be made more directly upon the visual mechanism itself rather than indirectly through changes in oxygen consumption or pulse rate.

Summary.—1. In this investigation the basal metabolism and pulse rate of 21 subjects were compared while reading for 1 hour under 1 footcandle of illumination compared with reading for 1 hour under 50 footcandles. On a succeeding morning the experiment was repeated with the order of illumination reversed. The experiments were carried out in an air-conditioned dark room where the temperature and humidity could be kept constant. No apparent relationship was observed between the basal metabolic rate (within the limits of ± 5 percent) and the level of illumination.

2. There was a general tendency for the heart rate to decrease slightly during the 2-hour reading period, irrespective of the level of illumination.

3. It is questionable whether reliable criteria for determining adequate levels of illumination for tasks such as reading during short periods of time (approximately 2 hrs.) can

be obtained in terms of oxygen consumption and heart rate, since the amount of tissue involved in the eyes and brain is relatively small in relation to the total amount of tissue in the body. The changes, if any, might be expected to be relatively small and consequently concealed by uncontrollable variables.

(Manuscript received May 1, 1939)

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PERFORMANCE OF THE EINTHOVEN GALVANOMETER WITH INPUT THROUGH A VACUUM TUBE MICROVOLTMETER

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In undertaking an investigation of the steady bioelectrical potentials found in the eyes of human subjects the author sought a suitably sensitive and rapidly acting direct current measuring instrument. It was essential that the measured potentials should be uninfluenced by the resistance at the electrodes and by current drainage. The problem was solved by employing an Einthoven galvanometer in a circuit which included a vacuum tube microvoltmeter used as input, thus causing the string galvanometer to perform as an electrometer. The combination has proved convenient and stable, and since it is applicable to more general use it seems to merit description. The discussion will include: a review of the characteristics of the string galvanometer, and brief mention of some earlier applications of electron tubes to use with this instrument; a description of the circuit developed in the present research; a presentation of illustrative records; and finally a discussion of the performance characteristics of the string galvanometer used as an electrometer.

The Einthoven galvanometer is singularly well adapted to the study of many bioelectrical phenomena because of its high degree of current sensitivity and its quick deflection time. In the better made instruments, insensibility to mechanical vibrations, ease of substituting different silvered quartz strings, wide range of proportionality of deflection, high-aperture optical magnification and direct photographic recording, and accessibility for adjustment combined with ruggedness of construction are features that recommend themselves to research workers. The characteristics of the

string galvanometer were described by Einthoven (1, 2, 3) and have been studied by several others, notably by Williams (4). Current sensitivity in this instrument is greatest when the string is made very slack and under such conditions it may reach 10^{-11} amp./mm with an optical magnification of 1000 times. Wertheim-Salomonson (5) working with an aluminum string 56 mm long, 0.002 mm diameter, in a field of about 16,000 gauss found a sensitivity of 2.5×10^{-12} amp./mm but the deflection (one-half period) required about 20 seconds. For high current sensitivity alone the Einthoven is therefore not the galvanometer of choice since is it more expensive than moving coil galvanometers of equal sensitivity and speed. However, from the standpoint of versatility for general use the high sensitivity possible in the string galvanometer is certainly advantageous. In the original instrument, current sensitivity, when high speed deflection at critical damping was preserved, was given (1) as about 10^{-8} amp./mm with a one-half period deflection speed of 0.009 sec. The larger string galvanometers commonly in use in hospitals and physiological laboratories in the United States [1] under favorable adjustment possess similar characteristics: approximately 2×10^{-8} amp./mm with a speed of 0.004 sec. William's detailed paper (4) should be consulted by those interested in using these instruments near their limit of deflection speed.

Forbes and Thacher (6) pointed out in 1920 that the use of thermionic valves in combination with the string galvanometer caused the deflections to be relatively independent of resistance in the input circuit and therefore made the galvanometer operate essentially like an electrometer. The advantage of being able to measure tissue potentials of prolonged or brief duration without voltage drop due to the circuit necessary for the recording, is of obvious importance for quantitative work along several lines.

The high resistance of strings ordinarily employed, 3000 to 10,000 ohms, partly achieves this desirable end, especially

[1] Designed by Dr. H. B. Williams (4) and designated as Type A. These string galvanometers were originally made by Charles F. Hindle and later by Cambridge and Paul Instrument Co. of America, New York City. Three hundred or more of them have been manufactured and distributed.

when care is taken to reduce and standardize the electrode resistance. Unfortunately in repeated experiments the tissue resistance is found to vary, and of course it is not constant between different subjects or tissue specimens. To meet these difficulties and at the same time to improve the factors of speed and sensitivity, Forbes and co-workers (**6**, **7**, **8**) in more recent studies have adapted vacuum tube amplifiers to the string galvanometer. By this means and through the use of very short strings they have greatly expanded the field of usefulness of this instrument for the study of action currents in nerve and muscle. But since there are several other fields of bioelectrical investigation in which amplification of tissue currents is not necessary, and where the string galvanometer is in general a highly suitable measuring instrument, there appears to be a need for a circuit providing for the simplified use of thermionic valves to insure a minimum of current drainage from tissues, and maximal freedom from the effects of resistance at electrodes. For this possible purpose the vacuum tube microvoltmeter recently designed and described by my colleagues Professors H. S. Burr, C. T. Lane and L. F. Nims (**9**) naturally suggested itself. It seemed probable that this microvoltmeter would make the good points of the string galvanometer more readily realizable in routine use. Therefore the task of combining the two instruments in working relationship was undertaken.[2]

The circuit finally developed to meet our research needs is shown in Figure 1, and in the accompanying legend some of the technical details and specifications are given. So far as these specifications relate directly to the Burr-Lane-Nims microvoltmeter they are mostly taken from the original publication (**9**). Section I in the diagram represents the test specimen, electrodes and selector switch, by means of which potentials from different tissues and combinations of tissues are connected for measurement. Section II, enclosed within

[2] In this connection I wish to express my obligations to Drs. H. S. Burr, C. T. Lane and L. F. Nims for their generous suggestions and advice at many points, and also to my laboratory assistant, Mr. A. R. E. Chapanis, for his painstaking help both in building up the apparatus and in routinizing its use. Dr. Lane kindly supervised the building of two vacuum tube microvoltmeters for me.

a broken line, is the compensation-potential and standardizing unit. This is the usual control box developed for electrocardiographic purposes and ordinarily used with these string galvanometers. In addition to a string protecting switch, S3, the unit provides standardizing potentials (R3 gives 9 steps of 10 m.v., R4 gives 15 steps of 1 m.v., and R5 gives 9 steps of 0.1 m.v.), useful for calibrating deflections and also for compensating, by means of the reversing switch S2, the body potentials usually present in the specimen. Section III, enclosed within a broken line, represents the Burr-Lane-Nims microvoltmeter unit, in which T1 and T2 are RCA Type 112 tubes. The connection passing most directly to the test tissue is taken to the T2 grid; *i.e.*, the 'hot lead' at S5. The other connection from the tissue including as it does the compensation-potential feature of the circuit is taken to the grounded or 'cold lead' on the microvoltmeter, and the grid resistance, R18, between these two leads and in series with the tissue under measurement is 5 megohms. Section IV of the diagram indicates the measurement recording and accessory apparatus. By means of S9 the output potential is first examined with a reflecting galvanometer, G, of 1.5 sec. deflection time; if necessary compensating potential is introduced to bring the reflected beam to zero position. Then S9 is moved into connection with the string galvanometer, S.G., and after standardizing or rechecking the deflection in terms of 1 millivolt with R4 the outfit is in readiness for recording measurements of tissue potentials.

As indicated in the diagram, the metal housings of II and III, the 'cold lead' and the frame of S.G. are grounded. Also shielding on the wiring between the units and that leading to the electrodes is connected to the ground terminal. It has not been found necessary to shield switches S4 and S9 or the patient, and since one side of the input circuit is grounded the patient is not. The small 110 volt, 60 cycle motors used to operate the long paper photographic camera and the timing mechanism (both not illustrated in the circuit diagram) must be well shielded and earthed to avoid so far as possible stray pick-up interference from them to the input circuit.

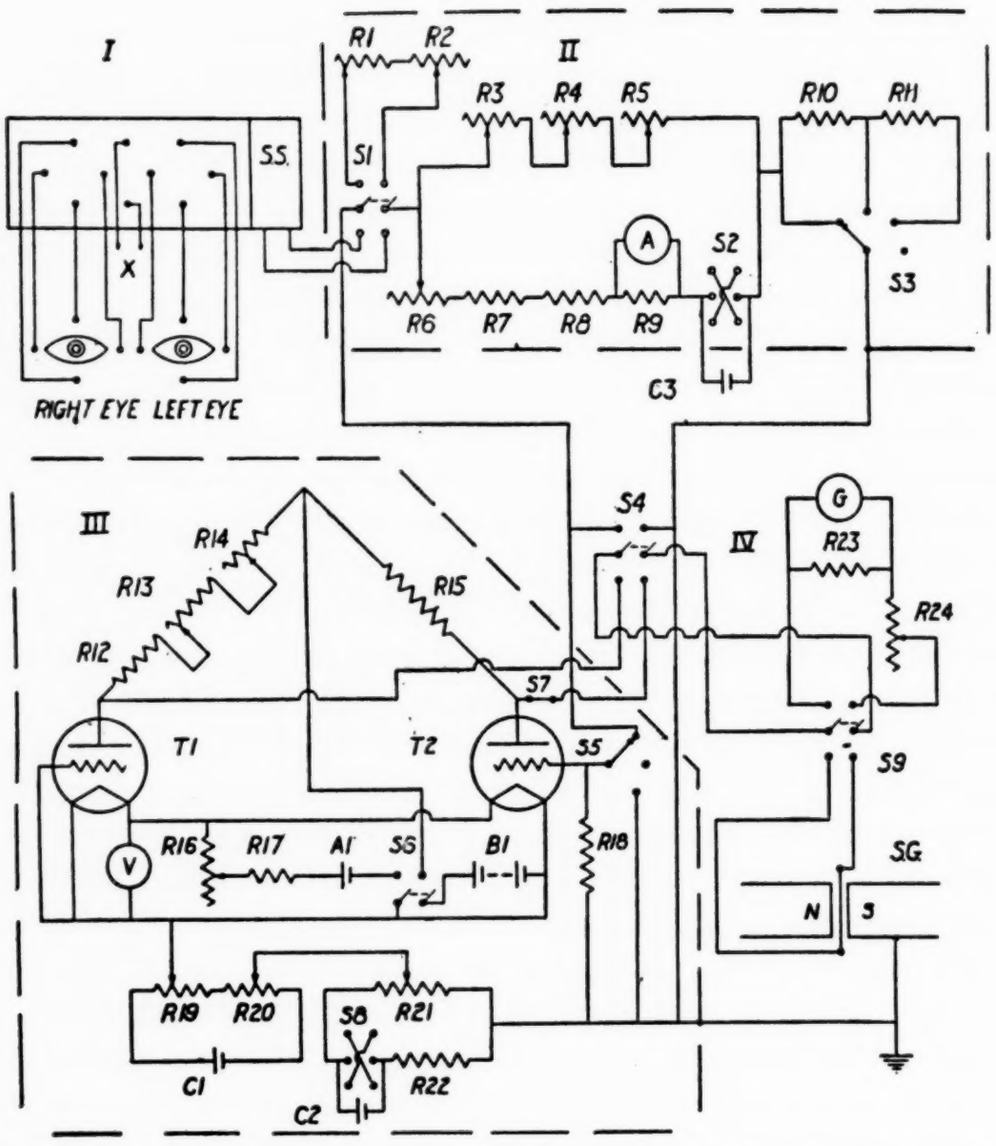

FIG. 1. A circuit for the string galvanometer used in combination with a vacuum tube microvoltmeter.

I Patient, electrodes, jack box, selector switch.
II Standardizing unit.
III Microvoltmeter (Burr-Lane-Nims type).
IV Galvanometers and associated apparatus.

A Microammeter for compensation adjustment.
V Filament voltmeter for microvoltmeter.
G D'Arsonval galvanometer for preliminary adjustment, sensitivity approximately 12×10^{-7} volt/mm at 1 meter, the distance used, and with oscillation time (half period) of 1.5 sec.
S.G. String galvanometer, Type A, for recording.
X Head electrodes.

A1 6 volt storage battery (Willard SPW 15–3).

B1 45 volt heavy duty dry cells.
C1 3 volt C battery, flashlight cells.
C2 1.5 volt C battery, flashlight cells.
C3 1.5 volt dry cell for compensation potential.

R1 Comparison Resistance 9000 ohms.
R2 Comparison Resistance 900 ohms.
R3 Compensation Resistance coarse adjustment, 9 steps of 100 ohms each, total 900 ohms, 10 millivolts per step.
R4 Compensation Resistance medium adjustment, 15 steps of 10 ohms each, total 150 ohms, 1 millivolt per step.
R5 Compensation Resistance fine adjustment, 9 steps of 1 ohm each, total 9 ohms, 0.1 millivolt per step.
R6 Standardizing Resistance, 9000 ohms, 9 steps.
R7, R8 Standardizing Resistances, fixed, 5000 ohms each.
R9 Meter shunt resistance, 25 ohms.
R10 String galvanometer protection resistance, 25,000 ohms.
R11 String galvanometer protection resistance, 50,000 ohms.
R12 5000 ohms, fixed, Electrad wire-wound.
R13 2500 ohms, variable, Zero adjustment coarse, General Radio Co. type 214.
R14 25 ohms, variable, Zero adjustment fine, General Radio Co. type 301.
R15 10,000 ohms, fixed, IRC wire-wound.
R16 25 ohms, variable, filament control, General Radio Co. type 301.
R17 5 ohms, fixed, filament resistance.
R18 5 megohms, grid resistance.
R19 600 ohms, variable, General Radio Co. type 314.
R20 20,000 ohms, variable, General Radio Co. type 314.
R21 200 ohms, variable, Compensation control, Dial calibrated 10 steps of 1 millivolt each, General Radio Co. type 314.
R22 30,000 ohms, fixed, IRC wire-wound.
Note: R12 to R22 are within microvoltmeter.
R23 64 ohms, Galvanometer shunt.
R24 Galvanometer resistance.
10 steps of 10,000 ohms each.
10 steps of 20,000 ohms each.
(10,000 ohms ordinarily used.)
S.S. Yaxley selector switch unit.
S1 Double pole, double throw, rotary switch.
S2 Double pole, double throw, rotary switch (Reverses compensation potential).
S3 Tap switch, galvanometer protection.
S4 Double pole, double throw switch.
S5 Tap switch, General Radio Co. with special amber insulation, input to microvoltmeter.
S6 Double pole, single throw, toggle switch.
S7 Single pole, single throw, toggle switch.
S8 Double pole, double throw, rotary switch, General Radio Co.
S9 Double pole, double throw switch.
T1, T2 RCA type 112A tubes with bases removed, packed in glass wool.

When input to the Einthoven galvanometer is through the microvoltmeter a reflecting galvanometer, G, is essential for adjusting compensation potential which, when the electrodes are applied to the skin, is frequently 10 m.v. or more. The usual means for protecting the string and for ascertaining the compensation requirements is by introducing resistance in the string circuit with S3. Thus R11 with 50,000 ohms, or the 25,000 ohms of R10, when placed in series with the string keeps the deflection small and well within scale protecting the string and making clear the polarity direction in which the compensation, if needed, should be introduced. But these protection resistances have slight effect on a circuit that already includes 5 megohms. When using the microvoltmeter it is desirable therefore to use S3 as represented in Fig. 1 and to include the string galvanometer in the output circuit by means of S9 only after the output has been monitored by means of a less delicate galvanometer.

The entire outfit is compact, accessible and easily kept in order. The string galvanometer is always ready for use as ordinarily employed or in combination with the microvoltmeter by simply moving one switch, S4. Except during hot humid weather the stability of the set has been good.

In taking measurements on more than 150 patients, adults and children, in the last few months, the instrument has not failed, and no string has been lost during these routine procedures. One or two strings have been sacrificed in the course of trying out various circuit combinations. Duplicate layouts for two-channel recording mounted on one concrete pillar are illustrated in Fig. 2. The two large string galvanometers may be seen in the background; one of the microvoltmeters appears at the left, the camera and the second microvoltmeter at the right; the control switches S4 and S9 are in the center next the writing pad.

The pairs of records reproduced in Fig. 3 illustrate certain points in the performance of the string galvanometer with and without the microvoltmeter. They show: (*a*) the influence of added resistance between the microvoltmeter and the string, and when resistance is introduced ahead of the

microvoltmeter; (*b*) the influence of resistance in the immediate string circuit on deflection sensitivity; and (*c*) the effect of the microvoltmeter circuit on the strings' optical focus position. It was desirable in parts of this comparison to use a string of low resistance so that the total resistance in the immediate string circuit could be varied over a wide range. A string having a resistance of 510 ohms and a diameter of 0.007 mm was chosen for the comparison and most

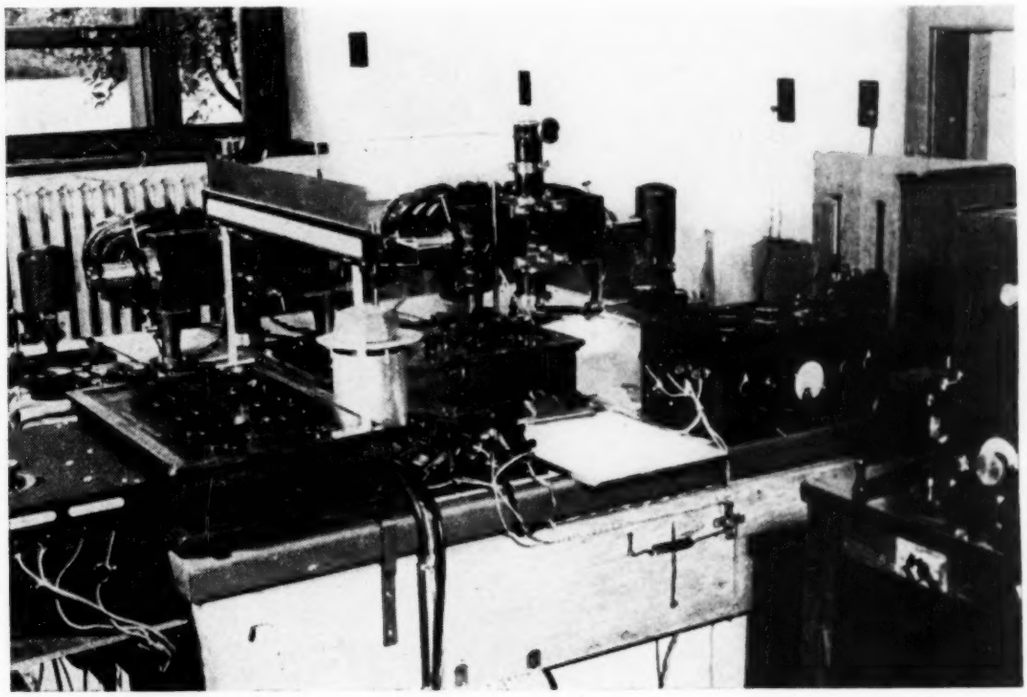

FIG. 2. Duplicate sets of string galvanometers and microvoltmeters for two-channel recording.

of the tracings reproduced in Fig. 3 were made with this string; the optical magnification was 1000 times. A non-inductive decade resistance and a high grade moving-coil microammeter were used in series with the string when A and A− were recorded, and the decade resistance only was used in C and C− and D and D−.

(*a*) Record A is with the microvoltmeter, and A− without it. The total resistance of the string circuit was about 600 ohms including the 50 ohm microammeter. The tension of the string was adjusted so that 10 m.v., one step on R3, through the microvoltmeter caused a 20 mm deflection,

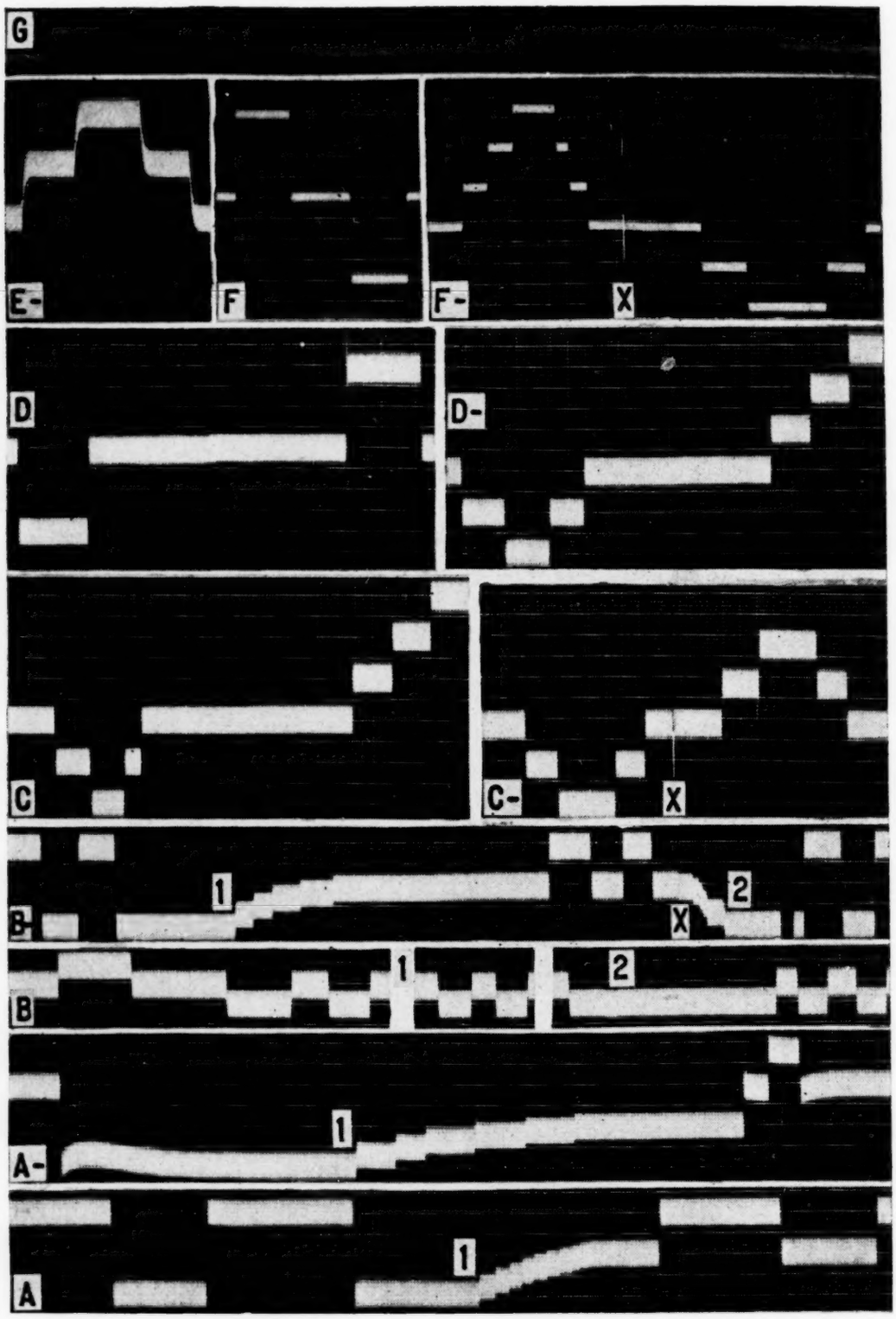

FIG. 3. A series of tracings illustrating the performance of the Einthoven galvanometer in combination with and without the Burr-Lane-Nims microvoltmeter. Records marked by a letter followed by a dash are for the string galvanometer without the microvoltmeter. The heavier vertical lines represent 0.2 sec intervals, and the heavier horizontal background lines represent 5 mm distances. The magnification of the optical system was 1000×. The curves are all to be read from left to right and in each the extreme left hand portion shows the base line position of the string's shadow. For further description see the text.

which may be seen in the left part of A. The meter reading was 3.4 microamps. Beginning at 1 in A, resistance of 10,000 ohms between the microvoltmeter and the string was introduced in ten steps of 1000 ohms each, thus decreasing the deflection from 20 mm to about 10 mm (1.8 microamps) as exhibited in the right hand portion of the record. No change in the tension of the string was made for A—, which immediately succeeded A. By shifting switch S4 the microvoltmeter was cut out of the circuit so that the string galvanometer could be used alone. Now, the application of 2.6 m.v. (in place of 10 m.v. as above) was found to give a deflection of 20 mm. The meter reading was 3.3 microamps. At 1, in the illustrated record a resistance of 600 ohms was added in series with the string in 6 consecutive steps of 100 ohms each, reducing the deflection from about 20 to 10 mm and the microamps from 3.3 to 1.8. In A—, movement of the string is very obvious as the moving coil of the microammeter takes up its position when the 2.6 m.v. is applied. There is no such movement of the string in A following the application of 10 m.v., showing that the string is less influenced by induction when used with the microvoltmeter, due to its relatively high output impedance of about 9,000 ohms.

B and B— show the effects on the deflection from adding resistance to the string circuit by means of R1 and R2 within the standardizing unit and thus ahead of the microvoltmeter. The left hand portion of B shows a deflection of about 5 mm from the application of 1 m.v. At 1 in the record, 500 ohms resistance was added by means of R2 with, as expected, no observable change in the deflection. At 2 the added resistance was taken out. The results are quite different in B—, without the microvoltmeter. The tension of the string has not been changed, and it will be noted that here 1 m.v. produces a deflection of 19 mm. Decrease in the deflection as resistance (500 ohms at 1 in the record) is turned into the circuit is evident, and the deflection is reduced to 10 mm. The record has been cut at X for purposes of reproduction. Withdrawal of the 500 ohms extra resistance is indicated at 2 beyond which the string deflection returns to 19 mm. Records A

and A−, and B and B− demonstrate that with a low resistance string circuit, approximately 600 ohms, the deflection for a given potential when using the microvoltmeter is approximately 25 percent of that found without it. The second point demonstrated is that introducing resistance ahead of the microvoltmeter does not modify the deflection amplitude.

(*b*) The influence on deflection sensitivity of resistance in the immediate string circuit is shown by illustrative records C to F− inclusive. In C and C− additional outside resistance was added so that the string circuit, outside the microvoltmeter, had a total of 3,000 ohms. Successive 1 m.v. increments have been impressed on the circuit in C, with the microvoltmeter, and in C− without it. Since the deflections in C− are just slightly smaller than for C it appears that a string resistance just lower than 3,000 ohms represents the value at which deflection amplitudes with and without the microvoltmeter will be equal. C− has been cut at X to shorten the record for purposes of reproduction. Both C and C− indicate that the deflections are not critically damped. Careful examination of the original records shows that the overthrow is about four times as large in the case of C as in C−. In D and D− resistance outside the microvoltmeter was added to bring the string circuit to a total of 8,000 ohms. In D, with the microvoltmeter, the string tension was adjusted so that 1 m.v. gave a deflection of about 20 mm. For D− no change in string tension was made, and 1 m.v. gave a deflection of 10 mm, or approximately 50 percent of that found with the microvoltmeter. From this pair of records it is evident that the microvoltmeter begins to act as an amplifier and gives a doubling of the deflection when the resistance of the string circuit reaches about 8,000 ohms. Record E− was taken with the string at the same tension as for D and D− but with all additional resistance (7,450 ohms) removed from the string circuit. Here increments of 0.1 m.v. gave overdamped deflections of 12 or 13 mm. The deflections in F and F− are from increments of 1 m.v. applied to a string .002 mm diam. with 7,000 ohms resistance. They are in line with the results found in D and D−. Record F illustrates the

representative conditions ordinarily used in this combination instrument.

(*c*) Illustrated record, G, in Fig. 3 shows the slight change of focus position for the string when used with the microvoltmeter circuit. Beginning at the left in G the microvoltmeter is disconnected and the string goes out of focus; reestablishing connection brings the string back into focus. This cycle is repeated in the right-hand part of the record. The difference in focus position shows particularly with small diameter strings. It is due to the fact that the string, centered in its 2 × 2 mm housing, is several volts above the grounded galvanometer frame which supports and surrounds it, and that the surrounding metal is not equidistant from the string on all sides. The projection objective of the microscope in the Einthoven instrument has a working distance of 0.2 mm. Here the grounded metal comes closest to the string and attracts it in true electrometer fashion. The upper end of the string is connected through R15 (10,000 ohms) with B1 (45 volts) as is its lower end through R12 (fixed 5,000 ohms), R13 (variable 2,500 ohms) and R14 (variable 25 ohms). On direct measurement with the string dismounted the upper string terminal showed 4.1 volts between itself and ground and the lower terminal gave 8.7 volts. These potentials only approximate the true working values since the resistance of the voltmeter was 2,500 ohms, which is less than that of the usual string employed. A .002 mm string at usual working tension of 1 cm per 1 m.v. is found to be deflected forward toward the projection objective by about .016 mm as measured in terms of the microscope focusing mechanism. At a magnification of 1000 times, this bowing of the string toward the nearest metal of its grounded housing is equal to a deflection of 1.6 cm, that is, it is equal to the effect from 1.5 m.v. applied to the string in the usual manner through R4 and R5. This bowing potential is present and constant whenever the microvoltmeter is in use with the string and constitutes a factor in its resting tension as it does in the restoring force after deflection.

Certain features of the performance of the string galvanometer in combination with the Burr-Lane-Nims microvoltmeter have been illustrated by means of records and described in the foregoing discussion. These characteristics, most of them directly predictable in terms of the circuit, need not be restated but should be supplemented by a brief presentation of one other factor which is that of deflection speed. The Einthoven instrument without supplementation has a quick deflection time as noted above. The half period represented by the off-zero phase is usually accomplished more promptly than is the half period of the to-zero phase. Some measurements of representative records will illustrate this. Einthoven (1), 1903, Table III, Fig. 4, reproduces a series of critically damped string galvanometer deflections. By means of a microscope comparator relatively accurate measurement of the deflection speeds for these tracings is possible since the time lines represent intervals of 0.004 sec. Deflection times in one direction (assumed to be off-zero) average 0.00840 sec while those in the other direction average 0.00927 sec. The ratio of off-zero to to-zero is therefore 90.6 percent. In Fig. 5 of the same reference a series of slightly less than critically damped deflections is illustrated. Deflection times for the assumed off-zero direction average 0.00614 sec, the return deflections average 0.00632 sec, ratio 97.1 percent. Williams (4), page 356, Fig. 7*a*, reproduces a record showing critically damped tracings in which the deflection time in one direction measures 0.00420 sec, and in the other, assumed to be the return to zero, it is 0.0041 sec, ratio 95.2 percent. For less than critically damped deflections Williams' Fig. 7*c*, page 357, shows speeds of 0.00129 sec and 0.00134 sec, ratio 96.2 percent.

My own data are in line with tendencies found in these earlier records but show longer deflection times. The results may best be given in tabular form, and stated in milliseconds. At the head of each column the number of millivolts used and the amplitude of deflection in millimeters are given. Each value in Table 1 represents an average of several deflections and all results are for the string galvanometer used without the microvoltmeter except the last column to the right. The

speeds are of course shortened in the under and lengthened in the over damped tracings, but the ratios remain fairly constant for both conditions. On the other hand, critically damped deflections for the string galvanometer used alone tend to give rather low ratios. Fortunately this characteristic in large part appears to be corrected when the input to the string is through the microvoltmeter. The ratio now reaches 98 percent or better and also the deflection speed is reduced by about 3 m.s. which amounts to 20 percent of the usual time value.

It is well known that the string galvanometer under favorable adjustment will give proportionality of deflection for 5 cm or more on either side of the zero position. Examination

TABLE 1

DEFLECTION SPEED IN THE STRING GALVANOMETER

Values in milliseconds

Deflections	8 m.v. 40 mm. U.D.	4 m.v. 40 mm. C.D.	2 m.v. 40 mm. O.D.	1 m.v. 40 mm. O.D.	1 m.v. 10 mm. C.D.	1 m.v.* 30 mm. C.D.
Off-zero.....	7.9	14.1	31.9	73.9	13.5	11.2
To-zero.....	8.4	15.2	33.4	76.9	15.0	11.4
Ratio.....	94.0	92.8	95.5	96.1	90.0	98.3

* With the microvoltmeter.

U.D., C.D., and O.D. represent respectively the conditions under damped, critically damped, and over damped.

of a series of such deflections from successive 1 m.v. steps of applied potential shows that the deflection times become progressively shorter the farther the deflection steps are from zero. For example, if the first 1 cm step gives 13.5 m.s. for the off-zero speed, the 5th proportional 1 cm step may show a reduction to 10.0 m.s. This instrumental tendency for the deflections to be more rapid when the string is already under some deflection tension may account in part for the increased speed found when the microvoltmeter is used. Under the latter condition, due to the potential difference between the string and the grounded galvanometer frame, as noted above in connection with G of Fig. 3, the string at zero is already at a deflection position (forward) equal to what 1.5 m.v.

would produce. It therefore may be assumed to respond somewhat more promptly just as the second step is quicker than the first in a proportional series. Another reason for the more rapid deflection of the string when used with the microvoltmeter probably arises from decreased electromagnetic damping. To the string circuit the microvoltmeter, as designed here, adds the equivalent of about 9,000 ohms (the output impedance of this instrument), which amounts to doubling the resistance and consequently reducing electromagnetic damping.

There are other and more effective ways of shortening the deflection time of the Einthoven than by using it with a thermionic valve circuit. The real objective in this study was to arrange a sensitive and rapidly acting direct current instrument in which resistance at the electrodes and current drainage would not operate unfavorably to influence measurements of bioelectrical potentials. Quite incidental to achieving this useful objective (**10**) it has been found that, rather than sacrificing good instrumental assets, the string galvanometer's operation is somewhat improved, if increased speed is a consideration, by combining it with a vacuum tube microvoltmeter used as input.

(Manuscript received December 21, 1938)

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ON BINOCULAR SUMMATION AT THRESHOLD[1]

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One form of evidence in favor of binocular summation has been derived from experiments in which it has been reported that the intensity threshold for two eyes is lower than the threshold for either eye alone. Results of this nature are by no means unequivocal, and the existence of binocular summation has been the subject of controversy for many years. This is due to apparent contradictions in what seem to be well-controlled investigations.

Quantitative results have been obtained by Cook (**2**) and by Lythgoe and Phillips (**7**), who find evidence of summation with binocular observation; and by Wölfflin (**10**), Abney and Watson (**1**), Müller (**8**), and Graham (**4**), who find a similarity of monocular and binocular thresholds. Other studies, some of which contain qualitative observations, are reviewed by Cook (**2**), and by Lythgoe and Phillips (**7**). The number of investigations lending support to each side seems to be about equal.

The present paper is a report of three experiments designed to yield further evidence on the matter. An attempt was made to control all influencing factors, especially those having to do with fixation. We used the method of determining thresholds for each eye and for both eyes after a constant period of dark adaptation which was preceded in turn by a constant interval of light adaptation. In none of the experiments was the threshold for both eyes lower than the threshold for the 'better' eye alone.

One of the most elusive and difficult factors to control in this type of investigation is fixation. The best general discussion of fixation requirements is in a paper by Hewson (**6**), and

[1] Supported by a grant from the Carnegie Corporation to Professor C. H. Graham under whose direction the research was performed.

several other writers point out considerations which no adequate investigation can overlook. Using various wavelengths of light, Abney and Watson (1) tested the relative sensitivity of two areas of the retina, one at the fovea and the other at 2.5 degrees in the periphery. The results from one observer show that the ratio of parafoveal sensitivity becomes as much as 27 to 1, with violet light (p. 126–127). This differential sensitivity of the retina makes imperative an exact control of fixation and the area of the retina to be stimulated.

The importance of carefully controlled fixation is emphasized by Cook (2). This investigator plotted the course of dark adaptation for each eye and for both eyes by measuring the time required for certain known intensities to reach threshold. Artificial pupils were used, through which the stimulus subtended an angle of about 55 min. Four small triangles of luminous paint bounded a small area used for fixation, so that small eye movements caused changes in the number or relative brightness of these spots. According to the author, this device was very effective in ensuring good fixation. Under these conditions, Cook found a considerably lower binocular threshold (about 50 percent) in three out of six subjects. In the remainder, the monocular and binocular thresholds were approximately equal.

A criticism of Cook's experiment is proposed by Hewson (6), who points out that Cook's apparatus is so constructed that neither the light-adapting screen nor the fixation area is at the same distance from the eye as the stimulus itself. This condition tends to induce a parallel position of the eyes, especially in monocular observation, when regard with the eye in this position without a doubling of the image is possible. In binocular observation the doubling of the image may serve as a corrective cue. In addition, the fact that the visual angles subtended by the light-adapting lights and the stimulus were not the same indicates that light passing from the former through the artificial pupils would cause adaptation of only a portion of the necessary retinal area. These two factors taken together may account for the summation found. This would be particularly true if the errors led to stimulation of a more sensitive portion of the retina in binocular observation.

Hewson's own experiment (**6**) yields additional evidence on the importance of fixation. In summarizing her experiment she states that "cases of apparent summation disappeared as subjects learned to secure fixation during a practice period" and "the length of the practice period necessary was shortened by increasing the intensity of the fixation light and restricting exposure time." Presumably the size of the fixation point would also enter to determine the goodness of fixation; fixation for a small point is more variable than fixation for a large.

In a recent investigation, Lythgoe and Phillips (**7**) find evidence of binocular summation by an amount which is slight compared with those reported in previous studies.[2] In this experiment a small red cross in the center of the stimulus patch served as a fixation point and light adaptation was accomplished by uniformly illuminating the chamber in which the subject sat. Under these conditions of illumination the subject probably focusses his eyes for far vision. It is possible that in the change from light adaptation to dark adaptation the parallel position of the eyes would suffice for the monocular determinations, while, for the binocular, a greater amount of convergence and accommodation would be necessary. This would lead to the stimulation of different retinal areas, and the slight amount of summation found (about 0.15 log unit of intensity) might be accounted for in these terms. This is a form of one of the criticisms made by Hewson concerning Cook's experiment, and it emphasizes the necessity for controlling fixation conditions.

The present investigation was planned with a view to controlling fixation factors. No attempt was made to record the entire course of dark adaptation under binocular and monocular conditions. Instead, a number of measurements were taken after a given interval in the dark, or at approximately the same point on the dark adaptation curve. The

[2] Since this paper has gone to press a new report (CROZIER, W. J. AND HOLWAY, A. H., Theory and measurement of visual mechanisms. II. Threshold stimulus intensity and retinal position, *J. Gen. Physiol.*, 1939, **22**, 351–364) has appeared which indicates a binocular summation effect of about the same magnitude as that found by Lythgoe and Phillips.

determinations were restricted to the fovea, where dark adaptation is rapid, and the point was always on the final steady level of the curve.

Experiment I.—In the determinations listed as Experiment I we made use of the apparatus diagrammed in Fig. 1*A*. The source of illumination, *S*, is a 200-watt

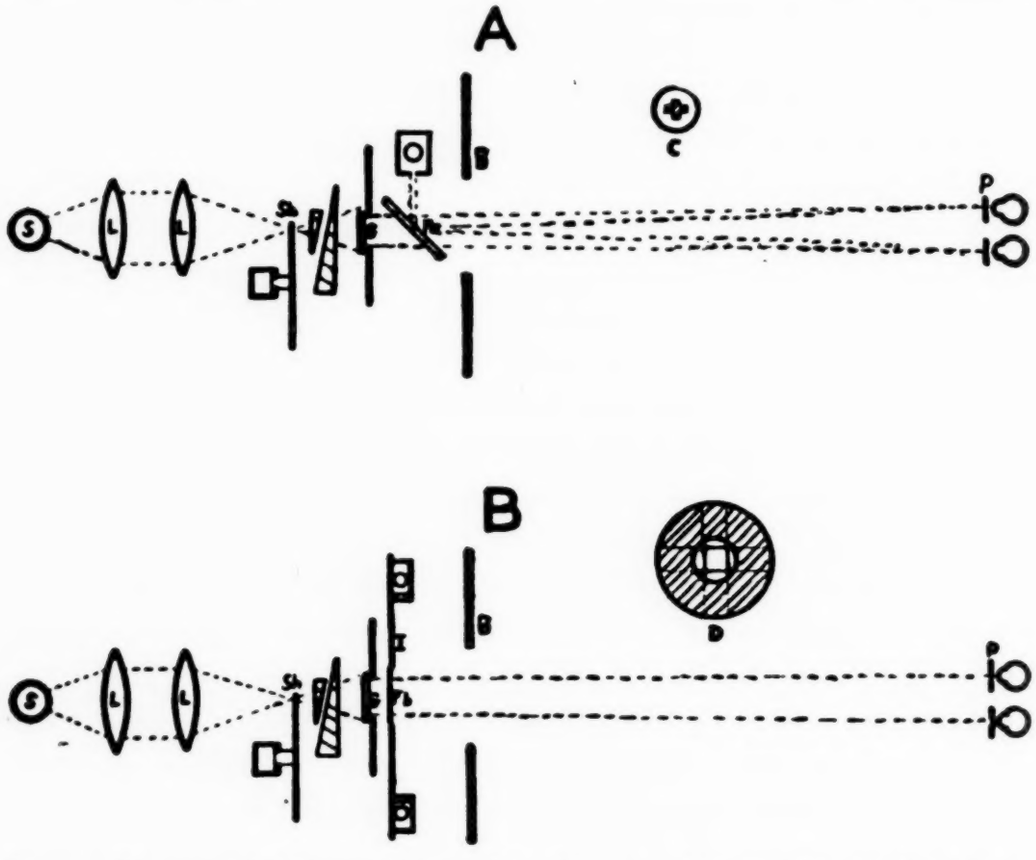

FIG. 1. Diagram of the apparatus used in these experiments. Description in text. Apparatus *A* was used in Experiment I; Apparatus *B* in Experiments II and III.

projection lamp operated at 110 volts dc. Light emanating from an aperture in the shield surrounding this lamp traverses a system of lenses, *LL*, coming to a small sharp focus in the plane of a rotating disc 59 centimeters away. This disc, attached to the shaft of a synchronous motor, serves as a shutter, *Sh*, and admits the light through a sector cut in its periphery for eight milliseconds. After encountering the shutter, the light passes through a hand-operated shutter and beyond this a neutral-tint wedge and its compensating balance wedge. The light beam finally emerges from the wedges and passes through a concave lens from which it illuminates a screen, *G*, comprised of two layers of ground glass. The glass screen is fitted into the wall separating the darkroom in which the experimenter controls the apparatus from the darkroom in which the subject is seated. On the subject's side of the screen, a cardboard sheet with a circular aperture, 40 millimeters in diameter, fits snugly between the glass screen and the wall. The diameter of this area subtends an angle of 60 minutes at the eye of the subject who is seated with his eyes at a distance of 230 cm from the screen.

Foveal fixation is achieved by the following apparatus. To the right of a sheet of clear glass, placed at an angle of 45 degrees to the observer's line of regard, is a small red cross, 20 mm by 20 mm, with arms 3 mm in width. The arms are cut in cardboard and backed by red cellophane on opal glass which is illuminated by a small flashlight bulb. The glass reflects the light from the cross to the eye of the subject, the image of the red cross, *Fa*, appearing in the center of the stimulus area and exactly in the same plane. (This is true since the distance from the ground glass screen *G* to the point on the plane glass intercepted by the observer's line of regard is equal to the distance—18 cm—from the glass to the red cross itself.) The appearance of the image in relation to the stimulus area is demonstrated in Fig. 1*C*. The red cross as seen subtends a visual angle of 30 minutes. Its brightness is 0.08 millilamberts, and in hue it is scarcely discriminable from the stimulus light which is provided by Wratten filter 71*A*. The real cross is shielded from the view of the subject by means of a screen, *B*.

The subject regards the fixation object through artificial pupils 2 mm in diameter. This diameter is sufficient to meet the requirements enumerated by Troland (9). In front of each artificial pupil is a hinged screen which can be raised by the experimenter so as to allow illumination of either or both eyes.

Light adaptation is effected by illumination of the stimulus screen with white light at an intensity of 15 millilamberts. Dark adaptation curves measured following this illumination disclose that a final stable level of adaptation is reached after about one and a half minutes in the dark.

For each subject the procedure included an initial five-minute period of dark-adaptation followed by nine series of threshold determinations. Each series was made up of (*a*) a period of light-adaptation for 15 seconds, (*b*) dark-adaptation for 115 seconds (during which interval the subject maintained fixation upon the cross), then (*c*) a rapid single threshold determination for each eye individually and for both together. To exclude any factor due to a constant order of measurement, all six permutations of the order for the three measurements were used with each subject.

For one of the subjects, the thresholds were determined by measuring the light intensity at which he first reported seeing light as the intensity was gradually increased from below threshold. For the other three subjects, a descending order was used. The flashes appeared at two- or three-second intervals, and as the experimenter was careful not to move the wedge far from the liminal range for a given subject, rarely were more than five or six flashes presented before the threshold value was reached.

The results obtained on each subject by this procedure are presented in Table I. The table indicates the mean of the nine measurements for each of the three thresholds. The variability of the values is represented in the columns containing the average deviation of each threshold. In no instance is there a significant difference between the limen for both eyes and the one for the better eye.

Experiment II.—The fixation conditions reported for Experiment I are relatively unsatisfactory. In this experiment the only object visible was a small red cross; thus there was no background field to offer stability of distance cues. Autokinetic movements and uncertainty of regard were often reported by the subject, and it was some-

times necessary for the experimenter to delay a series of measurements in order to allow the subject to recover fixation. In order to improve on this condition we used, in Experiment II, what we consider to be more natural and adequate conditions of observation. Reference to Fig. 1*B* will indicate the changes in apparatus.

The reflecting glass of Fig. 1*A* is removed and a cardboard background screen, *I*, is substituted at a distance of ten centimeters from the stimulus plane *G*. In the center of the screen is a circular aperture 110 millimeters in diameter which subtends a visual angle of three degrees at the eye of the subject. The cardboard area is illuminated, at grazing incidence on either side, by a small light in a box faced with opal glass. Under these conditions the brightness on the background is of the order of 0.015 millilambert. Two parallel cotton threads, 2 cm apart and perpendicular to a second pair of such threads, traverse the circular area. The square outlined by their intersections is rendered more strongly visible by a coating of white pigment. The fixation lights are screened by black shields, *B*, 6 cm in front of the fixation screen, with the inner edge of each screen 6 cm from the edge of the circular aperture. Under

TABLE I

LOG INTENSITY[3] FOR MONOCULAR AND BINOCULAR THRESHOLDS TOGETHER WITH THE AVERAGE DEVIATIONS, EXPERIMENT I

Order of Measurement	Subject	Log Intensity for Threshold					
		Right Eye	A.D.	Left Eye	A.D.	Both Eyes	A.D.
Ascending	M	$\bar{1}.07$	0.08	$\bar{1}.13$	0.06	$\bar{1}.07$	0.06
Descending	R	$\bar{2}.84$	.12	$\bar{1}.07$	.11	$\bar{2}.90$	.08
Descending	A	$\bar{2}.97$	.07	$\bar{1}.02$	.09	$\bar{2}.94$	.07
Descending	Gr	$\bar{2}.95$	.07	$\bar{2}.97$	.06	$\bar{2}.94$	.09

these conditions of fixation the subject regards what appears to be a dimly illuminated rectangular area, in the center of which is a black circular expanse; in the center of this expanse is the outline of the white square formed by the crossed threads. Fig. 1*D* shows a front view of the apparatus as seen by the subject. For the threshold determinations the red light of the stimulus (Wratten filter No. 70) appears in the center of the black circle enclosing the white square as indicated in Fig. 1*D*. In this experiment the subject fixates upon the outline of the dim white square and reports on the appearance of the red light. Since the distance between the threshold object and the crossed threads is a negligible part of the distance between the threshold object and the eye of the subject, the threshold object, when it appears, is seen in the same plane as the threads.

Preliminary light-adaptation for fifteen seconds was given to each subject before each series of limen determinations. This was provided by an illuminated opal glass screen (36 × 25 cm) at a distance of sixty centimeters from the subject. This surface offered a brightness of 350 millilamberts.

Other than the change in method of light-adaptation, the procedure used was the same as in Experiment I. Determinations showed that dark-adaptation following light-adaptation to the intensity of 350 millilamberts was nearly complete after a

[3] Intensity values for all the tables are in terms of millilamberts. The values were obtained by heterochromatic photometry against white light. The A.D. values are the average deviations of the log intensity readings.

minute and a half in the dark. For this reason we used the same interval between light-adaptation and threshold measurement as in the previous experiment—115 seconds. No artificial pupils were employed in this series.

The results are presented in Table II. As in the previous experiment there is no significant difference between the

TABLE II

LOG INTENSITY FOR MONOCULAR AND BINOCULAR THRESHOLDS FOR EXPERIMENT II WITH AVERAGE DEVIATIONS

Order of Measurement	Subject	Log Intensity for Threshold					
		Right Eye	A.D.	Left Eye	A.D.	Both Eyes	A.D.
Ascending	Gr	$\bar{2}.29$	0.06	$\bar{2}.26$	0.09	$\bar{2}.19$	0.08
Ascending	M	$\bar{2}.39$	.05	$\bar{2}.29$	.03	$\bar{2}.27$	.06
Ascending	A	$\bar{2}.35$	.04	$\bar{2}.33$	.03	$\bar{2}.34$	.04
Ascending	J	$\bar{2}.26$	.06	$\bar{2}.27$	.05	$\bar{2}.23$	.05
Ascending	L	$\bar{2}.09$	.05	$\bar{2}.00$	.07	$\bar{2}.00$	.08
Descending	Gr	$\bar{2}.21$	.08	$\bar{2}.22$	.07	$\bar{2}.27$	.06
Descending	Ga	$\bar{3}.93$	.10	$\bar{3}.89$	.08	$\bar{3}.86$	.07
Descending	B	$\bar{2}.01$	.03	$\bar{2}.05$	.05	$\bar{3}.99$	.02
Descending	V	$\bar{2}.13$	.07	$\bar{2}.17$	.07	$\bar{2}.11$	.05
Descending	K	$\bar{2}.11$	.06	$\bar{2}.08$	.07	$\bar{2}.05$	.09

threshold for both eyes together and the threshold for the better eye alone.

Experiment III.—Experiment III was performed in order to guard against the possibility that pupillary changes might have contributed to the negative results of Experiment II. Experiment III was therefore performed with apparatus, conditions of observation and procedure the same as in Experiment II except that artificial pupils, 2 mm in diameter, were employed.

The results of Experiment III are given in Table III and are essentially the same as those for Experiment II. There

TABLE III

LOG INTENSITY FOR MONOCULAR AND BINOCULAR THRESHOLDS WITH AVERAGE DEVIATIONS. ARTIFICIAL PUPILS EMPLOYED, EXPERIMENT III

Order of Measurement	Subject	Log Intensity for Threshold					
		Right Eye	A.D.	Left Eye	A.D.	Both Eyes	A.D.
Ascending	C	$\bar{1}.12$	0.04	$\bar{1}.07$	0.03	$\bar{1}.07$	0.03
Ascending	B	$\bar{1}.08$	.03	$\bar{1}.06$	.03	$\bar{1}.07$	.03
Ascending	Ga	$\bar{2}.94$	.06	$\bar{2}.99$	.06	$\bar{2}.95$	.05
Descending	Gr	$\bar{2}.68$	.06	$\bar{2}.68$	.06	$\bar{2}.72$	.06
Descending	A	$\bar{2}.82$	.07	$\bar{2}.80$	.07	$\bar{2}.81$	.05
Descending	M	$\bar{2}.83$	.05	$\bar{2}.87$	.09	$\bar{2}.87$	.04

is no evidence that the binocular threshold is lower than the threshold for the better eye.

Discussion

The results of our experiments constitute evidence against a binocular summative effect from stimulation of both retinas when the stimulus is at a threshold value. They show that there is no measurable lowering of the threshold with binocular observation, provided, of course, that the conditions of light- and dark-adaptation assure a constant level of sensitivity.

These results, however, are not interpreted as excluding central interaction effects under certain other experimental conditions. For instance, when the stimulating areas are illuminated with intensities far above threshold, there occur such effects as those encountered in Fechner's 'paradoxical experiment' (**5**, p. 522). A subject looks at an evenly illuminated white surface with both eyes, and then with one eye alone. In the latter condition the brightness of the surface appears reduced. However, by introducing various filters in front of one eye during binocular observation, it is possible to make the surface appear either brighter or darker when the subject shifts to monocular observation. These results have been validated by DeSilva and Bartley (**3**) who used an exact technique.

One possible condition for the occurrence of binocular summation is the use of long, or often repeated, exposures. This condition is not present in our experiments. Such a procedure was employed in the experiment of Cook (**2**), where the stimulus light remained on from the beginning of the period of dark-adaptation until it was reported as visible by the subject. The procedure of Lythgoe and Phillips (**7**) also involves presentation of the stimulus over relatively long periods, each exposure being of short duration. It may be that a central summation affects the threshold when the stimulus is active over fairly long periods of time, or against a background of cumulative retinal changes.

The present results do not remove the possibility of occurrence of binocular summation under particular conditions.

However, they do indicate one set of experimental conditions in which summation does not take place, and they re-emphasize the necessity for the control of such factors as fixation in experiments on binocular summation at threshold.

Summary

Three experiments are reported in which monocular and binocular intensity thresholds have been determined for the fovea, under constant conditions of light- and dark-adaptation. Special effort was made to control fixation conditions. No evidence is found for the existence of foveal binocular summation at threshold.

(Manuscript received January 3, 1939)

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REMINISCENCE IN RELATION TO DIFFERENTIAL DIFFICULTY *

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Previous studies of substance learning have reported divergent trends of retention for material which is learned by rote and for that which is learned more 'understandingly.' Numerous conditions have been varied but the association of the divergent trends (forgetting *vs.* reminiscence) with the related measuring instruments (Verbatim or V., *vs.* Summary or S.) persists. These studies are in terms of measures of central tendency. In a recent report (**3**) (which also summarizes other studies (**1**, **2**, **4**, **5**) as they bear on this point) English and Edwards point to the need for a more detailed analysis of the phenomena, having regard to particular responses rather than composite measures; they criticize the use of measures of central tendency in the investigation of reminiscence and forgetting or in any other case where relative differences are in question. Indeed a first analysis of 1,200 individual responses revealed the possibility that the reported trends of the means might be simply a function of differences in initial difficulty between the V- and S-items. A further analysis in which the items were weighted to equate for this complicating factor, however, gave evidence that the association of forgetting with V-items and the association of reminiscence with S-items was not an artefact or resultant of this difference.

While it is valid and legitimate thus to weight measures statistically, there is always danger of illicit assumptions and of cumulation of chance errors. It thus seems sounder procedure to eliminate, if possible, the effect of initial difficulty experimentally—that is, in this case, by the construction of a

* Much of the preliminary work upon which this report is based was done by W. P. A. workers under the supervision of Miss Juanita Lilliedale.

measuring instrument in which the V- and S-items are equal in initial difficulty. Should it be possible to construct such a test, there would be a valuable check on previous conclusions.

Subject Matter.—The material on which the measuring instrument was based was a modified passage from Melville's *Typee*, which proved unfamiliar to all our subjects. The passage was approximately 1,770 words long and required 10 minutes to read aloud. The first three paragraphs of the story are reproduced below.

Arrival at the Typees[1]

The maid and youth hurried along a path and we followed them until suddenly they set up a strange halloo, which was answered beyond the grove through which we were passing, and the next moment we entered upon some open ground at the end of which we spied a long, low hut, and in front of it were several young maidens. As soon as they perceived us they fled with wild screams into the adjoining thickets, like so many startled fawns. A few moments after the whole valley resounded with savage outcries, and the natives came running toward us from every direction.

Had an army made an invasion into their territory they could not have shown greater excitement. We were soon completely surrounded by a dense throng, and in their eager desire to behold us they almost stopped our progress; an equal number surrounding our youthful guides, who with amazing talkativeness appeared to be recounting the circumstances which had attended their meeting with us. Every piece of information appeared to redouble the astonishment of the islanders, and they gazed at us with questioning looks.

At last we reached a large and handsome building of bamboos, and were by signs told to enter it, the natives opening a lane for us through which to pass; on entering without ceremony, we threw our exhausted frames upon the mats that covered the floor. In a moment the slight building was completely full of natives, whilst those who were unable to obtain admittance gazed at us through its open cane-work.

The Test.[2]—The test which eventually met our requirements and revealed no significant difference in initial difficulty between the V- and S-items was of the True-False type, and contained 60 items, 30 of the S-type and 30 of the V-type. Certain of the items based on the above three paragraphs of the story are reproduced for purposes of illustrating the differences between the S- and V-items. It may be seen upon inspection that the S-items cover somewhat longer portions of the text than the V, are phrased in different words, and are more dependent upon an understanding of the content of the passage. The V-items, on the other hand, cover much shorter portions of the text, reproduce almost the exact words of the passage, and are less dependent upon an understanding of the content.

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[1] The addition of the *s* to Melville's word 'Typee' was a typist's error; we here follow copy as used.

[2] It may not be out of place here to remark that we are using test in its general sense, not in the narrow sense which has grown up around the 'mental test.' The lifted weights of the Weber-Fechner experiments and the nonsense syllables of Ebbinghaus no less than our true-false items are tests in this general sense.

SUMMARY ITEMS

2. When a stranger arrived in their midst the Typees people always kept respectfully at a distance.....T F
7. When they first arrived among the Typees Toby and Tom were tired out from their adventures.....T F
60. The inhabitants of the island never assembled in crowds.....T F

VERBATIM ITEMS

4. In front of the hut were several maidens.....T F
31. As soon as they perceived Toby and Tom the young maidens fled with wild screams.....T F
33. Those who were unable to gain admittance to the slight building could not gaze at Toby and Tom.....T F

Reliability of the Test.—A group of 118 elementary school children in the fifth and sixth grades read and simultaneously heard read the story. Immediately after the reading they were given the True-False test. Thirty days later and without prior warning they were retested. The range and distribution of scores on both the immediate (T_0), and the later (T_1) test proved the tests to be discriminatory. The reliability of the test by the split-half technique was figured twice with different combinations, each coefficient serving as a check on the other. The corrected split-half reliability coefficients for the whole test were .79 and .72. The test-retest coefficient (30 days) was .72.

Comparison of Means.—It is instructive to note first what we should find by following the usual procedure. Here we have a sixty-item recognition test for retention given immediately after study and 30 days later. How great is the 30-day loss? Well, the mean score for T_0 is 40.05 and for T_1 it is 38.59. The loss of 1.46 seems small but the critical ratio of the difference is well over 4.00 (the required figure)—showing that it is reliable. Conclusion: "Logical or substance-learning shows a slow but reliable decline when measured by a recognition test!"

We know in this case, however, that the measuring instrument is heterogeneous; we *made* it to consist of two kinds of items. Whether in other cases measuring instruments are truly homogeneous even when we use nonsense syllables or multiple-T mazes is another matter; here the heterogeneity is intentional and, to be fashionable, operationally defined.

At any rate, let us divide our data according to the two kinds of test items.

The same slow forgetting appears in the V-type half of the test. Literally the same! For the S-items show no loss at all; the means for the first and second testings are 19.55 and 19.52. *All the forgetting shown in the whole test is localized in the V-type half.* This no longer surprises us; we have repeatedly proved that we can construct tests which will produce this sort of result. We do insist that it is suggestive of a needed analysis of other tests of learning. We are confident that such an analysis will reveal more than a little heterogeneity in the conventional measures of learning, that some of this accidental heterogenetiy can be seen as the same in character as that deliberately imposed in our measures, and that it explains much of the inconsistency of result which clutters up the pages of our journals. Purified measures are a grime goal in research and every step towards purification of measurement is a step in scientific advance. We believe our segregation of items is such a step.

Analysis by Patterns.—Although it is apparent that the mean score cannot show a reliable loss unless considerable forgetting is taking place, it is by no means clear that only forgetting occurs—as our fractionation of the total score shows. Forgetting—and its contrary, reminiscence—ultimately concern a specific response. One forgets in such a way as to be unable to give the correct reply in response to a definite question. It is these individual reactions we must study. And for reminiscence and forgetting it is the *pattern* of forms of combinations of successive responses to a single item which are important. These patterns are simply the possible permutations of Right (R) and Wrong (W) responses taken two at a time for the two testings: RR, RW, WW, and WR.[3]

Our 118 subjects reacting to 60 items at two testings give us 14,160 individual responses, grouped into 7080 pattern

[3] We do not, of course, hold that strictly every WR response pattern means that the individual has reminisced. Sometimes, of course, the R is just good guessing. (It is almost equally true that many W's are just poor guessing.) But with the number of responses used, considerable differences between the frequencies of WR in the V- and S- half tests are pretty sure to be reliable (as will be seen by the usual criteria).

responses, 3540 for each of our two kinds of test items. The frequencies of each pattern are shown in Table 1.

The critical patterns are those set off in a box. It seems clear that there is more forgetting in the V-items and more reminiscence in the S.

This appearance is, however, deceptive. There were 2432 correct first responses to particular V-items; to these items the same persons made 632 wrong responses on the second trial, a forgetting of 26 percent. But 609 wrong responses to S-items following upon 2306 correct ones at first also constitute 26 percent. There is, then, exactly as much forgetting in this test of S as of V; a result which flatly contradicts the conclusion based on differences in averages. Yet the differences, be it noted, fully meet the usual criterion of reliability.

TABLE 1

Pattern	V	S	Total
RR	1,800	1,697	3,497
WW	654	671	1,325
RW (Forgetting)	632	609	1,241
WR (Reminiscence)	454	563	1,017
	3,540	3,540	7,080

We do not, of course, challenge the validity of the statistics either of means or of reliability of differences. It is true, exactly as set forth, that the mean of one group shows a greater loss than can be accounted for by chance, while the second group shows no mean loss. *The trouble is that we are depending on means when we should not.*

When we look to the remaining patterns the discrepancy is explainable. We find that 1108 first responses to V's were wrong; 454 or 40.99 percent were correct or were reminisced at the second test. But the S's yield an even higher 'reminiscence' score since 563 out of 1234 responses—45.62 percent—show a change from W to R. It turns out, then, though it is not obvious from casual scrutiny of the percentages, that in the S half-test forgetting and reminiscence so exactly balance that no net change in group mean is found. In the V half-

test, however, the same amount of forgetting is not quite balanced by reminiscence and a small but reliable net loss in mean is registered.

The association of the dichotomy of forgetting and reminiscence with that of 'V' and 'S' can be tested from the portion of Table 1 set off in a box by means of the chi-square test. Chi-square does not measure the size of the correlation; it merely indicates the likelihood that the result is chance. Applying it to the four-fold grouping indicated, we find chi-square to be 8.85, more than twice the value needed to assure reliable correlation. There is evidently something about the V-items which is correlated with less reminiscence while the reverse is true of S-items.

We have shown above that this correlation is not due to differential difficulty as reflected in means. But neither is it due in important degree to differences in difficulty as they are reflected in individual response patterns such as we are here following through. The portions of Table 1 outside the box are also constituted by two dichotomies. Casual inspection shows that the 'V's' are somewhat easier, the 'S's' somewhat more difficult, but the application of the chi-square test to this part of the table where the forgetting-reminiscence dichotomy is ruled out yields only the non-reliable value of 1.72.[4]

Summary

1. A test of retention of sixty items given to 118 school children immediately after the learning period and again after 30 days showed the slow decline in means 'expected' with meaningful material.

2. Breakdown of the whole test into the two parts of which it was known to be composed showed that all the loss in mean score was localized in one-half of the test—the half characterized by a larger influence of rote memory. In the other half-test, no mean forgetting occurs. It is suggested that many other measures of learning are equally heterogeneous,

[4] That is, we are here testing for the validity of association of the dichotomy, S–V, with easiness as shown by double success, RR, as compared with difficulty as shown by double failure, WW. This association is shown to fall within chance limits.

possibly in respect to the very same variation by which our half-tests were operationally differentiated.

3. On the ground, however, that retention is a matter of individualized responses, not of averages or other composite scores, analysis was pushed in terms of particular responses categorized in patterns according as they were correct or incorrect. Study of the 7080 patterns revealed an equal amount of forgetting in the operationally defined half-tests—thus contradicting the conclusion which would be drawn from the mean differences of paragraph 2, if the analysis were, as usual, not pushed beyond the obtaining of reliable differences in means. The mean differences in the half-tests were shown to be due then, not as might seem to differences in the amount of forgetting but to the greater amount of reminiscence which was found in one of them.

4. This difference in reminiscence is not due to chance distribution nor is it a mere correlate of differences in difficulty between the two half-tests.

5. Once more—with an entirely new instrument and with different subjects—it has been proved possible *to influence reliably and materially the amount of reminiscence* by a systematic alteration of the task imposed. However, one describes or interprets the alteration of the task, its effect upon reminiscence is a clear-cut and definite fact. We can produce it upon demand.

Discussion.—When two measuring instruments yield consistently divergent results the only possible conclusion is that they are measuring different things. It is possible, of course, that the 'different thing' in question is a mere artefact of the test instrument or of its use. But when the same divergence appears again and again under many and diverse conditions, the most parsimonious explanation is that the allegedly unitary phenomenon being measured is itself heterogeneous.

We believe, therefore, that our results make it almost impossible longer to regard retention as a unitary variable. Just what happens between the period of overt learning and

the recall or recognition period we do not pretend to have discovered. The speculative hypothesis of 'consolidation' and the scarcely less speculative hypothesis of unwitting practice still leave us with the problem of accounting for the differences in our half-tests. Whatever accounts for the forgetting-reminiscence difference is associated in significant fashion with that which forms the basis for our differentiation of measuring instruments.

Except in terms of how we actually made our tests, it is not easy to say what the differentiation is—a fact which throws into relief both the value of operational definition and its limitation to the superficial level. Our S-items may certainly be characterized, however, as more meaningful, as requiring a more comprehensive grasp of what has been learned, as relying somewhat less upon rote memory and a mechanical sort of reproductive association.

It is still not clear how this difference (or these differences) are related to superior tendency to reminiscence. One very tentative and vague suggestion might be this: instead of a retained memory 'trace' suffering slow decay, we may have a process of active consolidation and integration during which the original 'traces' lose their independence and are transformed into a more comprehensive unit. It is, however, entirely possible that this active reorganization and enrichment of understanding is concurrent with 'decay,' more or less as traditionally conceived. Indeed, decay or loss of detail may even be a conditioning factor for the reorganizing process posited.

One familiar with the literature may see that many more or less isolated facts fall into some sort of plausible relation in the light of such an hypothesis. But a pretty hypothesis is valuable chiefly as a stimulus to research; isolated facts seldom lie close enough together to be synthesized without the aid of experimentation specifically directed towards filling the gaps. We believe that our work, if it has not filled many holes, has clearly pointed out where some of them are and indicated at least one way in which the figure may be closed.

(Manuscript received January 7, 1939)

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THE LENGTH OF THE WORK PERIOD AND THE WORK CURVE

BY J. E. BARMACK, PH.D.*

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In 1929, Bills and Brown (1) published an article on the influence of a 'time set' and 'amount set' on the initial rate of work. Subjects were instructed to work as quickly as possible with accuracy, in adding pairs of digits. With varying amount-limits of work, the initial rate of work was directly proportional to the number of problems to be completed. However, with varying time-limits of work, no comparable relationship was obtained between the initial rate of work and the duration of the work period. They used four time units of 2, 4, 8 and 16 minutes duration, and four amount units of 135, 270, 540, and 1,080 pairs of digits.

More recently, Krueger (2) had 36 subjects add pairs of digits under the two sets of conditions, viz., variable amount and time limits. He used larger and longer units of work than did Bills and Brown. The units which he used were (*a*) 5 minutes, (*b*) 25 minutes, (*c*) 50 minutes, (*d*) 500 problems, (*e*) 2,000 problems, (*f*) 4,000 problems. With this schedule, Krueger found that the initial rate of work was highest for the smallest amount of work, and the shortest work period. These results appear to be at variance with those reported by Bills and Brown. Conceivably, however, the difference in the results of the two studies may be a function of the units of time or amount used by different investigators. It is to be noted that the amount and time limits of Krueger are longer than those of Bills and Brown.

The present investigation arose from two interests. First, to study the relationship between initial rates of work and various long periods of work to determine which findings

* I wish to express my gratitude to Dr. J. G. Peatman, colleague, for his very helpful reading of the manuscript.

would be corroborated, those of Bills and Brown, or those of Krueger. Secondly, to analyze certain possible interrelationships between the work curve, feeling tone curves and the length of the work period.

Subjects and Procedure

Twenty-three students out of the twenty-seven who originally volunteered from classes in general psychology at the College of the City of New York were able to complete the experiment. There were five experimental sessions. The materials for the entire experiment consisted of mimeographed problem and subjective rating sheets. Each problem sheet contained one hundred pairs of six-place numbers to be added. There were six different sets of one hundred each, placed in series. The subjective rating sheets consisted of six nine-point rating scales of very bored to very much interested, very relaxed to very strained, very irritated to very pleased, very peppy to very fatigued, very sleepy to very wide-awake, very attentive to very inattentive. The subjects were given instructions on how to fill out the subjective rating scales and on the procedure of marking the problem last attempted, at the end of each fifteen-minute period. Then, for the first session, which was a practice and orientation session, they filled out the first subjective rating sheet with the six scales and worked for eight fifteen-minute periods. At the end of each period, they filled out another subjective rating sheet. An interval of approximately forty-five seconds was permitted in order to fill out the subjective rating sheet. In the remaining four sessions the subjects worked in the same room for four, eight, twelve, and sixteen, fifteen-minute periods.

The order of the four work periods for each individual was arranged so that no unit had a position of advantage. The sessions were held in a biology laboratory where individual lights for microscopic use were available in addition to good, general lighting. The seats were adjustable. Absolute silence during any experimental session was a prerequisite to participation. The cooperation of the surviving subjects was excellent. The experimental sessions took place in the evening, starting at six-thirty P.M. Each session was separated from the other by a period of one week.

Results

A comparison of the rate of work of the one-hour period with those of the first hour of the other work intervals provides equivocal results. The rate of work of the shortest period is fastest. The initial rate of work of the two-hour period is faster than that of the three-hour period, but only insignificantly faster than that of the four-hour period. The rates and critical ratios are provided in Tables 1 and 2.

Although only one of the critical ratios is greater than 3.0, the consistency of the differences in the rates of work of the first four fifteen-minute periods, for the one, two and three hour condition, is strongly suggestive that these are truly

TABLE 1

TOTAL NUMBER OF PROBLEMS COMPLETED DURING THE FIRST HOUR OF THE FOUR WORK PERIODS
(N = 23)

	One Hour	Two Hours	Three Hours	Four Hours
Number completed.....	661.5	620.9	603.5	617.8
σ av.....	27.1	24.7	24.5	24.8

TABLE 2

CRITICAL RATIOS OF THE DIFFERENCES BETWEEN THE RATES OF WORK DURING THE FIRST HOUR OF THE FOUR WORK PERIODS[1]

	Two Hours	Three Hours	Four Hours
One Hour.....	2.16	3.10	2.13
Two Hours.....	X	1.70	.14

[1] In computing the critical ratios, the standard formula for $\sigma_{diff.}$ was used:

$$\sigma_{diff.} = \sqrt{\sigma_{av_1}^2 + \sigma_{av_2}^2 - 2r\sigma_{av_1}\sigma_{av_2}}$$

significant differences. The initial rate of work during the four hour period is high and requires explanation.

The subjective and objective data are graphically represented in Figure 1. On this graph there are a few items of interest. The feeling tone judgments on each of the six rating scales become *uniformly unfavorable* for all four work periods despite the fact that there is no decrement in output during the one hour period. Thus, in three out of the four periods there is a relationship between changes in feeling tone and work output. Another item of interest is the fact that the feeling tone ratings for the four-hour period do not become unfavorable as rapidly as that of the two or three-hour period. The significance of this item will be discussed.

DISCUSSION

The results of this study provide evidence for the generalization that under the conditions of the experiment the rate of work of a one-hour period is greater than the first hour rates of a two, three, and four-hour period. The first hour rate of the two-hour period is somewhat greater than that of the

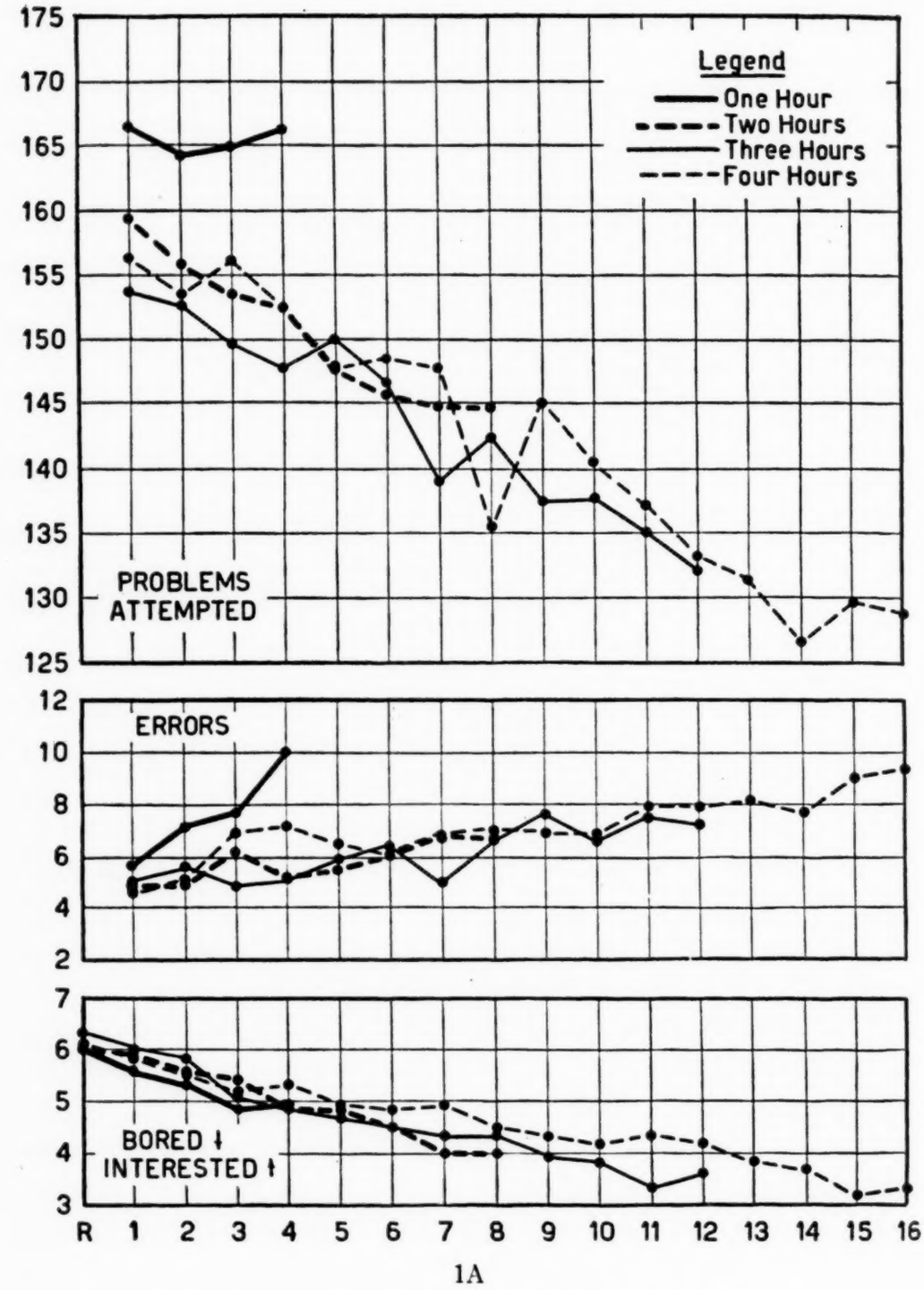
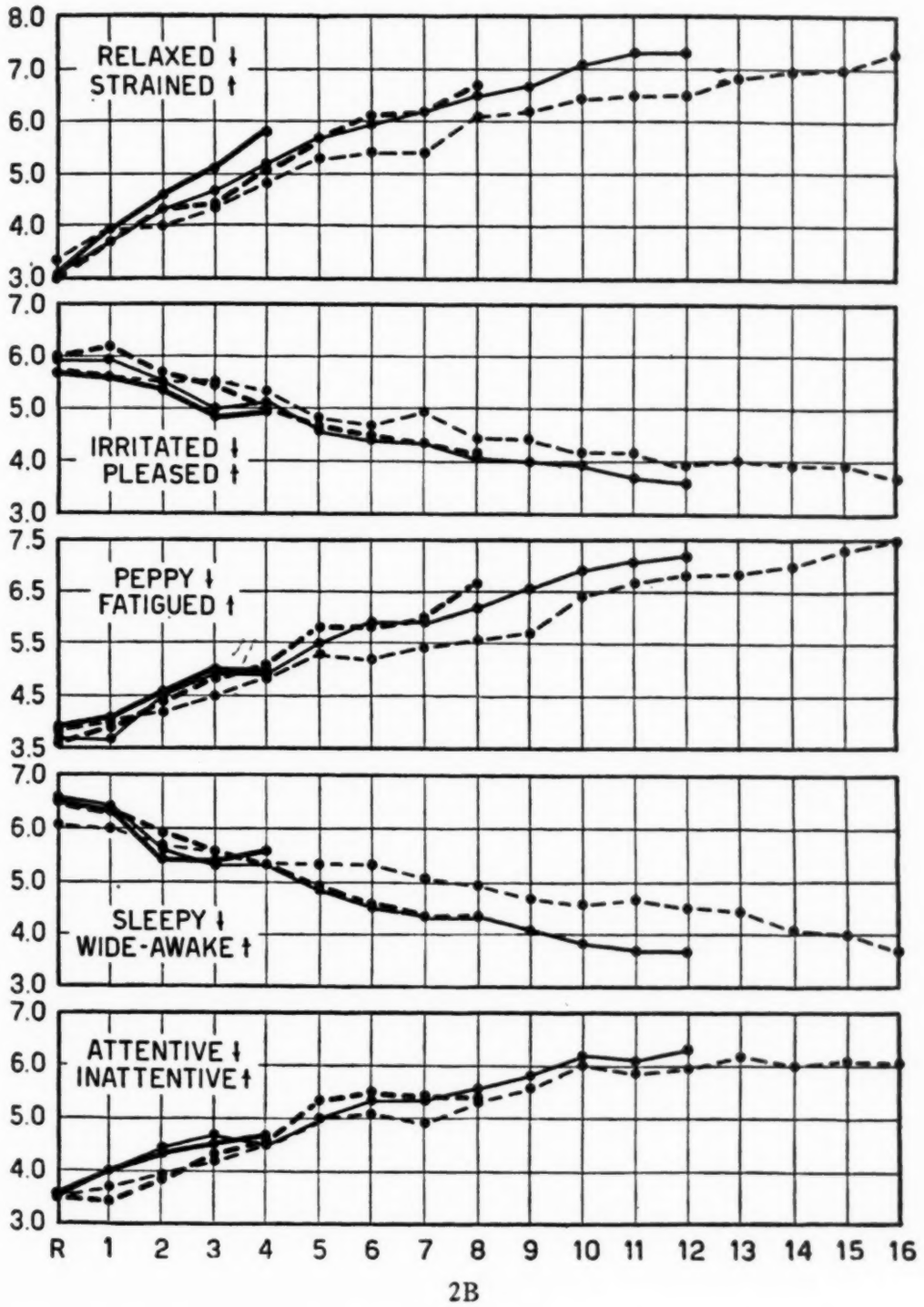


FIG. 1. The average work output and feeling tone curves of 23 subjects adding pairs of six-place numbers for one, two, three and four hours. *R* represents the period



2B

before activity, and 1, 2, 3, etc., represent 15-minute activity periods. All subjective ratings are on a nine-point scale. Arrows indicate the direction of increase.

three-hour period. There is evidence that the rate of work of the fourth period was partly determined by a special factor. The whole experiment is very obviously a gruelling task, the most challenging session of which is the four-hour period. Subjects apparently prepared themselves for this period. A significant comment by one of them was, "I just got set for that one, I felt that if my set were disturbed I couldn't go on." The subjective ratings for the four-hour period indicate that the development of an unfavorable feeling tone is not as rapid as for the two or three-hour period. This fact may be another indication of a more determined production set for this period.

On the whole, the evidence is in favor of the generalization that with relatively long work periods, the fastest rate will be obtained with the shortest period. The results of Krueger's work and the present investigation are not to be considered as contradicting the generalization of Bills and Brown on the effect of a 'time set' or an 'amount set' on the initial rate of work for two reasons. First, the generalization of Bills and Brown is restricted to the first quarter minute of work. Secondly, within the range of the shorter time or amount limits, one may be working with a different organization of motivational and other factors than obtains in the range of the longer ones. That is, within a range of various smaller units of work the desire may dominate to work quickly at the largest unit. However, within the range of various larger time or amount units, 'pacing' is virtually a necessity for the longest amounts or periods. These considerations point to the need for further experimentation with various quantity and time schedules as well as tasks, to determine at what critical points and under what conditions the generalizations of Bills and Brown, and Krueger hold.

Our data indicate that with a homogeneous task, there is a progressive change in feeling tone, unfavorable to the task for all four work periods. There is a corresponding drop in output in all but the shortest work period. This evidence merely adds to the already ambiguous character of the interrelationship between feeling tone and work output which is, that

under certain prescribed conditions (particularly with a homogeneous task) the relationship is close, but that work output and feeling tone may at times act as independent variables.

Summary and Conclusions

After a two-and-one-half hour practice period, twenty-three subjects added pairs of six-place numbers for four, eight, twelve, and sixteen fifteen-minute periods. At the end of each period they filled out a subjective rating sheet indicating their feelings of boredom, strain, irritation, fatigue, sleepiness and attentiveness.

1. It was found that the rate of work for the one-hour period was faster than the rate of the initial hour of the two, three and four-hour periods. The initial hourly rate of the two-hour period was greater than that of the three-hour period. The initial hourly rate of work of the fourth period was practically the same as that of the two-hour period. The latter fact is, in part, possibly attributable to the challenging nature of the task. Further work is suggested.

2. Although the results of this investigation confirm the findings of Krueger, certain reasons are given for the position that the results may not contradict the findings of Bills and Brown.

3. The feeling tone judgments of each of the six rating scales become progressively unfavorable for all four work periods. The work output correspondingly decreases in all but the shortest period. This evidence adds to the already ambiguous character of the interrelationship between output and feeling tone, viz., under certain conditions (particularly with a homogeneous task), feeling tone and work output may vary together; nevertheless, output may vary independently of feeling tone.

(Manuscript received January 5, 1938)

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STUDIES IN THE TRANSPOSITION OF LEARNING BY CHILDREN: III. TRANSPOSITIONAL RESPONSE AS A FUNCTION OF THE NUMBER OF TRANSPOSED DIMENSIONS

BY T. A. JACKSON

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This report is based upon experimental work done immediately following that reported in the second of this series of articles. The present experiment is essentially an extension of the previous one. The same apparatus and stimuli were used, and the same children were employed as subjects. For details of procedure the reader is referred to the earlier paper. For theoretical discussion of the problem, the first article may be consulted.

In the previous experiment all eight subjects were given two hundred trials of post-fixation practice on a discrimination habit. Twenty-five practice trials were given per day. The experimental group, five subjects, were given critical trials only after all two hundred trials of practice; the control group, three subjects, were given three critical trials at the close of each day of practice. Four-dimensional stimuli were used in both the training and the critical trials. Upon transposition of the stimuli, four subjects made a stable absolute choice, three made a stable relative choice, and one made inconsistent or unstable choices.

In the present experiment, only one or two of the four dimensions of the stimuli were transposed. The several sets of critical trials were interspersed with short periods of training on the original, four-dimensional training stimuli. The purpose of this was to maintain the practice level attained in the two hundred trials of practice. The six-day program through which each subject was put is shown in Table I. A description of the various sets of stimuli used in the critical trials

TABLE 1

SHOWING THE SCHEDULE OF TRAINING AND CRITICAL TRIALS, AND A DESCRIPTION OF THE STIMULI USED IN THE VARIOUS SETS OF CRITICAL TRIALS

Day	Number and Kind of Trials	Dimensions of the Description of the Two Stimuli*				
		Stimuli	Area	Shape	Brightness	Pattern
1st	5 Train.	Four	AB	AB	AB	AB
"	5 Trans.	Size	BC	SS	WW	NN
"	10 Train.	Four	AB	AB	AB	AB
"	5 Trans.	Shape	BB	BC	WW	NN
2nd	5 Train.	Four	AB	AB	AB	AB
"	5 Trans.	Brightness	BB	RR	BC	NN
"	10 Train.	Four	AB	AB	AB	AB
"	5 Trans.	Pattern	BB	RR	WW	BC
3rd	5 Train.	Four	AB	AB	AB	AB
"	5 Trans.	Size and Shape	BC	BC	WW	NN
"	10 Train.	Four	AB	AB	AB	AB
"	5 Trans.	Size and Brightness	BC	SS	BC	NN
4th	5 Train.	Four	AB	AB	AB	AB
"	5 Trans.	Shape and Brightness	BB	BC	BC	NN
"	10 Train.	Four	AB	AB	AB	AB
"	5 Train.	Shape and Pattern	BB	BC	WW	BC
5th	5 Train.	Four	AB	AB	AB	AB
"	5 Trans.	Bright. and Pattern	BB	RR	BC	BC
"	5 Train.	Four	AB	AB	AB	AB
6th	25 Train.	Four	AB	AB	AB	AB
"	5 Trans.	Four	BC	BC	BC	BC

* The letters 'A,' 'B,' and 'C' refer to the three stimuli used in a transposition experiment. The training is on 'A' and 'B' and transposition is made to 'B' and 'C.' In this work these letters represent the following dimensions for area, shape, brightness, and pattern:

	A	*B*	*C*
Area	1,250 sq. mm.	2,000 sq. mm.	3,200 sq. mm.
Shape (pointedness of triangle). When this dimension is used, the stimuli are triangles, and their pointedness is measured by the ratio of base to altitude	5/8	1/1	8/5
Brightness (three grays were arbitrarily chosen from a series of 0–30 (white to black) made by Stoelting)	No. 3	No. 15	No. 28
Pattern (this consisted of the *distance* between black lines (.8 mm. wide) drawn vertically on the stimuli)	2.5 mm.	4.0 mm.	6.4 mm.

The following letters mean: S = square stimuli; R = rectangle stimuli; W = white stimuli; N = no pattern in the stimuli.

is given in the same table. The schedule was planned so that all critical trials would follow immediately after practice on the original pair of training stimuli. This relation between training and critical trials had obtained in the first experiment, and it was considered important to carry it through the present work. The length of interval between practice and transposition is undoubtedly an important factor in determining the type of choice.

The procedure may be criticized on the basis of the possibility of cumulative transfer effects among the sets of critical trials. The writer will readily admit that a subject should not be given more than a single critical trial, if all possibility of transfer is to be eliminated. However, the difficulty of securing subjects, and the time required to train subjects up to a given practice level, make it necessary to give more than one critical trial and even more than one set of critical trials. In this experiment the possibility of any significant cumulative transfer effect will be checked by repeating the transposition of all four dimensions. A shift in the type of choice of this second set of trials would indicate cumulative transfer.

Results

Table II shows the type of response made by the various subjects in the several sets of critical trials In presenting these data, the subjects are classified according to their grouping in the previous experiment. The table also includes an indication of the type of response made by each subject in the four-dimensional transposition tests.

Before attempting any analysis of these results it may be well to review the three successive stages of transfer in the transposition experiment. The types of response which are associated with little, moderate, and much practice are unstable choice, stable relative choice, and stable absolute choice, respectively. The transfer effect in the first of these stages is small and makes for an unsystematic choice between transposed stimuli. In the two latter stages transfer may be regarded as being greater and consequently a systematic choice is made. In most of the following analysis it will be sufficient

TABLE 2

SHOWING THE TYPE OF CHOICE MADE BY EACH SUBJECT ON THE VARIOUS SETS OF CRITICAL TRIALS

Name of Subject	Grouping in Previous Exp.	Dimension or Dimensions Transposed											Number of Sets of Critical Trials in Which Each Type of Choice Was Made			
		One Dimension				Two Dimensions					All Four					
		Size	Shape	Brightness	Pattern	Size and Shape	Size and Brightness	Shape and Brightness	Shape and Pattern	Brightness and Pattern	Previous Experiment	Present Experiment	Unstable Choice (Stage 1)	Stable Relative Choice (Stage 2)	Stable Absolute Choice (Stage 3)	% Stable Choice 1 or 2 dimensions
El	Exp.	5R	5R	5R	3R*	5R	5R	5R	5R	5R	5A	5A	1	8	0	88.8
Th	Exp.	5R	5R	5A	3R	5R	5R	5R	5R	5R	5A	5A	1	7	1	88.8
Ma	Exp.	1R	5R	5R	2R*	5R	5A	5R	5R	5R	5A	5A	2	6	1	77.7
Er	Con.	5R	5R	1R	3R*	5R	†	5R	5R	5A	5A	5A	2	5	1	75.0
Fr	Exp.	3R*	5R	5R	2R*	5R	5R	5R	5R	2R*	5R	5R	3	6	0	66.6
Jo	Con.	4R	5A	3R	3R	5R	1R	5R	4R	1R	5R	5R	6	2	1	33.3
An	Con.	3R*	3R*	2R	2R*	3R*	3R*	3R*	2R*	5R	5R	*	9	0	0	0
Ed	Exp.	4R	2R*	3R*	3R*	4R	2R*	2R*	3R*	3R*	*	*	9	0	0	0
	Stable Choice	37.5	75.	50	0	75.	57.2	75	62.5	50	87.5	75				

* These choices indicated a position habit.
† No data.

merely to distinguish between unstable and stable choices, that is, between stage one and stages two and three taken together. Since there were five critical trials in each transposition test, an entry in Table II of 5R (five relative choices) or 5A (five absolute choices) will indicate stable choice; an entry of any number less than five will indicate unstable choice.

The results shown in Table II will be examined with special emphasis on the following questions. Which single dimension was most potent in transferring training? What is the relative effectiveness of single dimension as compared with two dimensions for producing transfer? What is the relative effectiveness of one or two-dimensional transfer as compared with four-dimensional transfer? How do the results of the subjects making absolute choice in the previous experiment compare with the other subjects? Was there any cumulative transfer among the sets of one or two-dimensional transposition?

The answer to the question regarding the relative potency of the various dimensions may be found in the lowest row of Table II. The percentage of subjects making stable response for the four dimensions, size, shape, brightness and pattern are 37.5, 75, 50, and 0.0 respectively. These values are based only upon the critical trials in which only one dimension was transposed. Essentially the same results are obtained when computation is based upon the critical trials involving two dimensions. In the latter case, as above, shape was found to be the strongest dimension and pattern the weakest.

A comparison of the effectiveness of transfer via one dimension vs. two dimensions may be found by computation of the average of the appropriate percentages in the last row of Table II. These are 40.6 percent for the single dimension and 63.9 percent for the two dimensions. The corresponding value for the four-dimensional transpositions is 81.2 percent. That transfer is proportional to the number of dimensions involved is not at all surprising.

We come now to a consideration of the relation between the results for those subjects who made absolute choices in the

four-dimensional transpositions and those who made relative choices. In this comparison only the first six subjects will be used. Application of the same criterion as used in the foregoing shows that the 'absolute-choice' group makes 81.4 percent stable choice in the nine sets of critical trials involving one or two transposed dimensions, and the 'relative-choice' group made 50.0 percent. It is obvious that the 'absolute-choice' group shows a greater amount of transfer effect. This result is consistent with the findings of an earlier study in which it was shown that absolute choice is a function of a relatively great amount of practice.

A qualitative analysis of these data will now be undertaken. The absolute-choice group tended, in their responses to the one and two-dimensional transpositions, to make relative choices. This result suggests a sort of regression, since relative choice normally occurs following less practice than absolute choice. The relative-choice group shows something of the same sort of regression in that they tend to make unstable choice. Such backward transition is more in evidence in the sixth subject than in the fifth. The results of the seventh subject are not as stable in general as those of the first six, still so far as they go, they too show definite regression from relative choice on the four-dimensional transpositions to unstable choice in all the one- or two-dimensional transpositions. The assumption may be made that a reduction of the number of transposed dimensions should cause one stage of regression (absolute to relative or relative to unstable). In 70.9 percent of the instances such regression appeared. If the same assumption be applied to the results of the first seven subjects but without the restriction of only one degree of regression, the percent increases to 80.7. The mechanism for this regression will be discussed later.

The question was raised earlier as to whether there might be some cumulative transfer from one set of critical trials to the next. If such were the case the results would probably be distorted. In order to make a rough check on this possibility the final sets of critical trials were given using four-dimensional stimuli. The results are shown in Table II. In

only one of the eight subjects did any such cumulative effect appear. Since this subject was a rather marginal case, our results as a whole stand as they were presented.

The choice time results are shown in Table III. It is evident that there is a sharp rise in choice time on the first critical trial. Through the sequence of five critical trials,

TABLE 3

SHOWING, FOR EACH SUBJECT, TWO SETS OF CHOICE TIME THROUGH THE SEQUENCE OF CRITICAL TRIALS

First is given the average choice time of the nine sets of trials in which one or two dimensions were transposed, and second the choice time for the final set of trials in which all four dimensions were transposed.

		Average for Nine Sets of Critical Trials in Which One or Two Dimensions Were Transposed					Final Set of Critical Trials in Which All Four Dimensions Were Transposed				
		Trial					Trial				
		1	2	3	4	5	1	2	3	4	5
Eleanor	2.5	6.9	2.8	2.3	2.2	2.1	4	1	2	1	2
Thomas	2.1	3.3	2.8	2.6	2.0	2.0	7	3	3	2	2
Marguerite	1.5	2.8	2.2	2.0	1.9	1.8	2	2	2	2	2
Ernest	1.8	3.4	1.8	1.4	1.2	1.4	4	2	2	1	2
Frank	1.7	3.3	2.2	2.0	1.7	1.9	3	1	1	1	1
Joyce	2.8	3.1	2.4	2.3	2.2	2.1	3	2	1	1	1
Anton	2.0	2.3	1.7	1.4	1.4	1.2	1	1	1	1	1
Eddie	1.3	2.7	2.1	1.7	1.5	1.7	1	1	2	1	1

however, it drifts back to the training level. Transposition apparently causes a sort of disruption of response and a readjustment is required. This disruption persisted through all sets of critical trials for most subjects. Since the theoretical implications of this increase in choice time have been discussed elsewhere (1) it will not be pursued here. Suffice it to say that these data give further support to the interpretation in the earlier report.

DISCUSSION

The foregoing results may be interpreted in terms of the theory of identical elements by merely pointing out that the greater the number of dimensions used in transposition, the greater the transfer effect. Although that explanation is

correct so far as it goes, this theory cannot account for many of the other facts and relations among the results. Also a satisfactory interpretation of the results cannot be made by appeal to the concept of 'equivalent stimuli.' To the writer the theory of generalization is most adequate for interpreting all these data. Only that theory attempts to 'explain' the occurrence of the absolute choice, and only that theory attempts to follow the changes in transfer effect as it is a function of the degree of practice.

According to this theory, the generalizations of the stimuli are reduced through practice. When that reduction goes beyond a certain point the response in the transposition trials will change from relative to absolute. However, if different stimuli were chosen for transposition, stimuli that were less disparate, such that would still fall within the gradually contracting limits of generalization, then the relative response would again be made. A reduction in the number of dimensions transposed, in the present experiment, is equivalent to a reduction in magnitude of the difference between two single-dimensional transposed stimuli. The implication in this analysis is that these four-dimensional stimuli are perceived merely the sum of four dimensions. The observed facts support this sufficiently well that it is without doubt largely true. However, the writer would not insist that a finer analysis of the stimuli used here would not reveal that perceptually they are 'more than' merely the sum of four dimensions.

Summary

Eight children, ranging in age from 46 to 57 months, were given two hundred trials of practice, beyond fixation, on four-dimensional stimuli (size, shape, brightness, and pattern). Four of the eight subjects made stable absolute choice in transposition trials following the practice. Three others made stable relative choice and one made unstable choice. (These results have been presented in an earlier report (**2**).) These same children were then given a series of nine sets of critical trials in which there was a reduction of the number of transposed dimensions. In the first four of these sets, only

one dimension was transposed; in the last five, two dimenssion were transposed. The sets of critical trials were interspersed with practice on the original stimuli in order to maintain the two-hundred-trial practice level. The results show a marked degree of 'regression' of response in the transposition trials. Those subjects previously responding absolutely on the four-dimensional stimuli now responded relatively, and those previously responding relatively now responded unstably. Choice time was markedly greater in the critical trials, particularly the first, than it was in practice trials.

(Manuscript received December 16, 1938)

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NEW APPARATUS FOR THE MEASUREMENT OF BODILY MOVEMENT

BY A. S. EDWARDS

University of Georgia

In the new laboratory of the University of Georgia some study has been made of bodily movement while the subject stood at attention for 15 minutes. Measurements have been made of movements at the feet and waist and head.

Movements of the feet consisted in the shift in weight from one foot to the other and measurements were made by modified use of the hypnograph. Movements of the head were quite unsatisfactorily measured by means of modified Veeder counter. This note is especially concerned with the measurements of the movements of the waist.

The new apparatus is homemade and seems to be without a source of error. It is in duplicate. One moves forward and the other backward.

This apparatus consists essentially of the following: A metal tube about 28 inches in length through which a stout cord can pass. The cord is tied at one end to the belt of the subject and after passing through the tube hangs with a light weight which keeps the cord taut.

An opening is cut near the center of the metal tube which exposes the cord of about 1¾ inches. A pawl is held in position by a short upright so that it falls at an angle of about 30 degrees to the cord. The essential point in relation to this apparatus is that the pawl permits the tube to be drawn in one direction but with no movement in the other direction. The measurement of the string thus exposed above the weight by the movement of the tube is an accurate cumulative measurement of the movement in one direction. The other similar apparatus on the opposite side of the subject gives cumulative measurement of the movement in the opposite direction.

The apparatus are supported by light structures which make a runway for the tube; a 12 inch piece of metal is brazed on to the under portion of the tube near the end to act as a keel.

It is necessary to use non-stretch cord. The essential features of the apparatus are shown in Fig. 1.

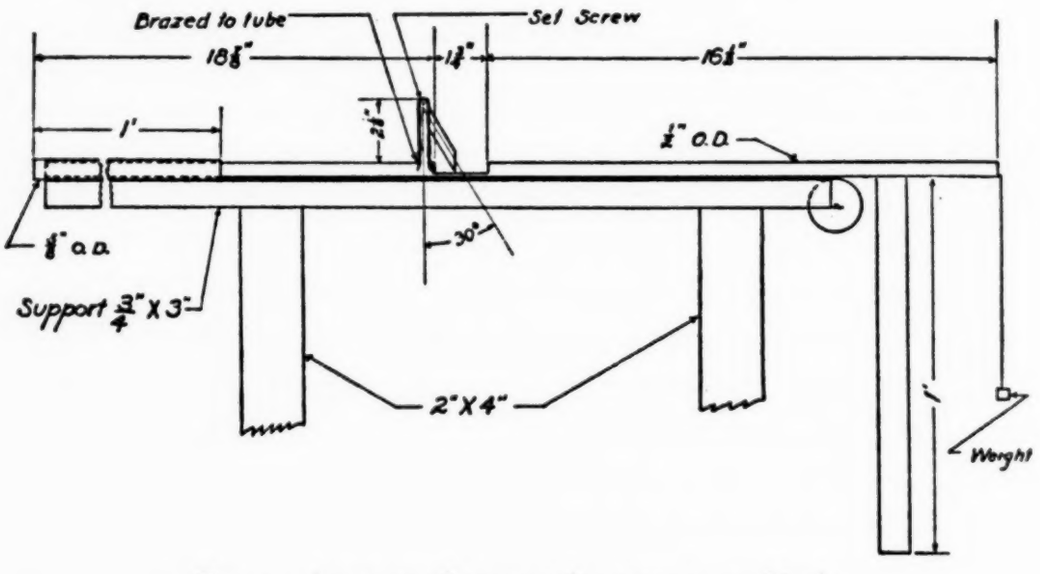

FIG. 1. Apparatus for measuring movement of body.

The tube works best when moving inside a larger tube and when a wheel is used to ease the movement.

(Manuscript received January 9, 1939)